MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXVI. *February* 9, 1866. No. 4.

THE Annual General Meeting of the Society:

WARREN DE LA RUE, Esq., F.R.S., President, in the Chair.

Arthur Brewin, Esq., 2 Copthall Chambers; and
Arthur Finch, Esq., Roupell Park, Streatham Hill,

were balloted for and duly elected Fellows of the Society.

The President then said:—

Before proceeding with the business before us, I would, for a few moments, bespeak your attention. You will recall the great regret felt by us all on learning that the Medal awarded last year to Professor Bond, of the United States, did not reach that country until after his lamented decease.

You will, doubtless, therefore, be much gratified now to learn that Professor Bond, though he did not actually receive the Medal, was, some time before his death, made aware of the honour that had been conferred upon him, and even of the grounds on which the award had been made. For, being very desirous of not omitting the mention of any of his numerous works, I early in January transmitted to him a rough copy of my intended address, with a request that he would kindly point out any omissions. Although himself too ill to reply, he, by his friend Lieut. Safford, expressed his heartfelt gratification at this recognition of his labours, while at the same time he confirmed the general accuracy and completeness of my proposed account of his researches.

Report of the Council to the Forty-sixth Annual General Meeting of the Society.

Progress and present state of the Society:—

	Compounders.	Annual Contributors.	Non-residents.	Patroness and Honorary.	Total Fellows.	Associates.	Grand Total.
December 31, 1864	163	264	24	5	456	49	505
Since elected ...	7	22	...	...	...	...	...
Deceased	−5	−3	−2	−1	...	−3	...
Name withdrawn	...	−1	...	...	...	...	...
Expelled	...	−1	...	...	...	...	...
Resigned	...	−7	...	...	...	...	...
Removals	2	−2	...	...	...	...	...
Dec. 31, 1865 ...	167	272	22	4	465	46	511

Treasurer's Report of Receipts and Expenditure for the year ending December 31, 1865:—

RECEIPTS.

	£	s.	d.	£	s.	d.
Balance of last year's account	...	...		281	10	11
By Dividend on £2100 Consols	30	14	3			
By ditto on £4700 New 3 per Cents	68	14	9			
By ditto on £2100 Consols ...	30	19	6			
By ditto on £5000 New 3 per Cents	73	15	0			
				204	3	6
On account of arrears of contributions ...	147	16	0			
164 contributions, 1865–6, 6–7	343	7	0			
9 compositions	189	0	0			
29 admission-fees	60	18	0			
21 first contributions	34	13	0			
				775	14	0
Sale of publications	...	...		73	13	0
Due to Assistant-Secretary — petty cash	...	...		0	1	1
				£1335	2	6

EXPENDITURE.

	£	s.	d.	£	s.	d.
Salaries :—						
Editor of Publications	60	0	0			
Assistant Secretary	100	0	0			
Commission on Collecting	33	0	0			
				193	0	0
Investments :—						
Purchase of £300 New 3 per Cents, 86¾	266	12	6			
Ditto £200 Consols, 89¼ ...	178	15	0			
				445	7	6
Taxes :—						
Assessed and Income	8	2	3			
Parish Rates	13	10	10			
				21	13	1
Bills : —						
Strangeways and Co., printers ...	206	3	4			
J. Rumfitt, bookbinder	2	19	8			
J. Basire, engraver	4	8	9			
Pearson, wood-engraver	1	7	0			
Turnor Fund	4	7	3			
Annual Dinner	7	6	0			
Law expenses	6	0	0			
Insurance	10	0	6			
				242	12	6
Miscellaneous items :—						
Books and parcels	4	19	1			
Postages	34	15	2			
House expenses	22	18	8			
Expenses of evening meetings ...	13	13	0			
Waiters attending meetings	3	17	0			
Coals and Gas	16	12	11			
Repairs	4	6	0			
Sundries	14	14	2			
Mrs. Jackson's annuity, 1 year ...	8	16	3			
				124	12	3
Balance at Banker's				307	17	2
				£1335	2	6

Audited and found correct, this twenty-sixth day of January, eighteen hundred and sixty-six, by us, the undersigned duly appointed Auditors of the Society,

JOHN BROWNING,
S. M. DRACH,
WILLIAM SIMMS.

Assets and Present Property of the Society, January 1, 1866:—

	£	s.	d.	£	s.	d.
Balance at Banker's				307	17	2
1 Contribution of 7 years' standing ...	14	14	0			
1 ,, 6 ,, ...	12	12	0			
2 ,, 5 ,, ..	21	0	0			
4 ,, 4 ,, ...	33	12	0			
4 ,, 3 ,, ...	25	4	0			
40 ,, 2 ,, ...	168	0	0			
40 ,, 1 ,, ...	84	0	0			
2 ,, various sums ...	10	15	0			
				369	17	0
Due for Publications of the Society				3	4	6
£2300 Consols (including the Lee Fund).						
£5000 New 3 Per Cents (including Mrs. Jackson's Gift).						
Unsold Publications of the Society.						
Various astronomical instruments, books, prints, &c.						
Balance of Turnor Fund (included in Treasurer's Account)				92	11	5

To the Council of the Royal Astronomical Society of London.

Gentlemen,—We, the undersigned, duly appointed Auditors of the Society's Accounts for the year ending 31st December last, have this day searchingly investigated the same, and find a cash balance at the Banker's in favour of the Society of three hundred and seven pounds seventeen shillings and twopence (£307 17*s.* 2*d.*), which includes one shilling and one penny petty cash overpaid by and due to your Assistant Secretary.

We find further that the Funded Stock now possessed by the Society amounts to five thousand pounds New Three per Cents, and two thousand three hundred pounds Consols, yielding dividends of £219 per annum.

We further beg to record our unanimous conviction that the Accounts of the Society, as presented to us, have been kept with unnecessary complexity; and we earnestly recommend a return to the simpler method formerly adopted.

As witness our hands, this twenty-sixth day of January, eighteen hundred and sixty-six.

S. M. Drach,
John Browning,
William Simms.

Royal Astronomical Society's Apartments, Somerset House, London.

The following Revenue Account and Balance Sheet, which are made out in accordance with the plan adopted for the year 1863, show the exact position of the Society both as regards assets and liabilities:—

REVENUE ACCOUNT FOR THE YEAR 1865.

	£ s. d.	£ s. d.	Dr. £ s. d.
To Composition fund	... ...	189 0 0	
Turnor fund	15 0 0		
Lee fund	3 5 6		
Mrs. Jackson	8 16 3		
		27 1 9	
Rent	... ...	— — —	
Editor	60 0 0		
Printing	145 8 6		
Engraving	8 15 9		
Binding	1 9 8		
Assistant-Secretary	100 0 0		
Collection	33 0 0		
		348 13 11	
Law	6 0 0		
Taxes	21 15 11		
Postage	34 15 2		
Parcels	4 19 1		
House Expenses	22 18 8		
Coals and gas	18 8 11		
Meetings	17 10 0		
Medal	10 10 0		
Insurance	10 0 6		
Repairs	4 6 0		
Sundries	22 2 2		
		173 4 5	
Sums written off	38 17 0		
To Capital—increase	500 5 11		
		539 2 11	
		£1277 3 0	

	Cr. £ s. d.
By New compositions	189 0 0
Composition fund—deaths	105 0 0
Admission fees	60 18 0
First year's contributions	34 13 0
Annual contributions	554 8 0
Carried forward	£943 19 0

		Brought forward	£943 19 0
Dividends, January	£30 14 3		
,, April	68 14 9		
,, July	30 19 6		
,, October	73 15 0	204 3 6	
Sale of publications	... ...	74 8 0	
By Investing under par	... ...	54 12 6	
		£1277 3 0	

BALANCES, December 31, 1865.

Dr. £ s. d.		Cr. £ s. d.
	Cash due to Assistant-Secretary ...	0 1 1
307 17 2	Banker's balance.	
	Proceeds of Turnor fund	92 11 5
	Proceeds of Lee fund	6 5 9
	Due to Mrs. Jackson	– – –
369 17 0	Annual contributions—arrears.	
	Paid in advance	6 6 0
8 8 0	Admission fees—4 due	
3 4 6	Due for publications.	
	Reserve against annual contributions due ...	65 0 0
2300 0 0	3 per Cent Consols at par.	
	,, Turnor fund	500 0 0
	,, Lee fund	110 0 0
5000 0 0	New 3 per Cents at par.	
	,, Jackson fund	300 0 0
	Estimated liabilities for work in progress	26 3 6
10 10 0	Medal in stock.	
– – –	Unsold publications.	
– – –	Books and instruments.	
	Composition fund ... £3507 0 0	
	Net capital 3386 8 11	6893 8 11
£7999 16 8		£7999 16 8

Before the other business of the Meeting was proceeded with, a discussion arose respecting the best mode of keeping the Financial Accounts and of presenting them annually to the Society, whereupon the following Resolution was duly proposed, seconded, and carried:—

"That this Society recommends that the Financial Statement be left exclusively in the hands of the Treasurer, who is to draw up the Accounts in conformity with the Bye-Laws."

Stock of volumes of the *Memoirs*:—

Vol.	Total.	Vol.	Total.	Vol.	Total.
I. Part 1	16	XI.	194	XXI. (together).	101
I. Part 2	57	XII.	201	XXII.	192
II. Part 1	75	XIII.	215	XXIII.	190
II. Part 2	39	XIV.	402	XXIV.	193
III. Part 1	91	XV.	183	XXV.	208
III. Part 2	110	XVI.	207	XXVI.	214
IV. Part 1	110	XVII.	185	XXVII.	472
IV. Part 2	122	XVIII.	185	XXVIII.	432
V.	137	XIX.	198	XXIX.	459
VI.	157	XX.	190	XXX.	216
VII.	180	XXI. Part 1 (separate).	316	XXXI.	198
VIII.	167			XXXII.	230
IX.	171	XXI. Part 2 (separate).	100	XXXIII.	258
X.	183				

The instruments belonging to the Society are as follows:—

The *Harrison* clock,
The *Owen* portable circle,
The *Beaufoy* circle,
The *Beaufoy* transit,
The *Herschelian* 7-foot telescope,
The *Greig* universal instrument,
The *Smeaton* equatoreal,
The *Cavendish* apparatus,
The 7-foot Gregorian telescope (late Mr. Shearman's),
The Variation transit (late Mr. Shearman's),
The Universal quadrant by Abraham Sharp,
The *Fuller* theodolite,
The Standard scale,
The *Beaufoy* clock, No. 1,
The *Beaufoy* clock, No. 2,
The *Wollaston* telescope,
The *Lee* circle,
The *Sharpe* reflecting circle,
The *Brisbane* circle.

The *Sheepshanks*' collection of instruments, viz.,—

1. 30-inch transit, by Simms, with level and two iron stands.

2. 6-inch transit theodolite, with circles divided on silver; reading microscopes, both for altitude and azimuth; cross and

siding levels; magnetic needle; plumbline; portable clamping foot and tripod stand.

3. $4\frac{6}{10}$ achromatic telescope, about 5 feet 6 inches focal length; finder, rack motion; double-image micrometer; object-glass micrometer; two other micrometers; one terrestrial and ten astronomical eyepieces, applied by means of two adapters.

4. $3\frac{1}{4}$-inch achromatic telescope, with equatoreal stand; double-image micrometer; one terrestrial and three astronomical eyepieces.

5. $2\frac{3}{4}$-inch achromatic telescope, with stand; one terrestrial and three astronomical eyepieces.

6. $2\frac{3}{4}$ achromatic telescope, about 30 inches focus; one terrestrial and four astronomical eyepieces.

7. 2-foot navy telescope.

8. 45-inch transit instrument, with iron stand, and also Y's for fixing to stone piers; two axis levels.

9. Repeating theodolite, by Ertel, with folding tripod stand.

10. 8-inch pillar-sextant, divided on platinum, with counterpoise stand and horizon roof.

11. Portable zenith instrument, with detached micrometer and eyepiece.

12. 18-inch Borda's repeating circle, by Troughton.

13. 8-inch vertical repeating circle, with diagonal telescope, by Troughton and Simms.

14. A set of surveying instruments, consisting of a 12-inch theodolite for horizontal angles only, with extra pair of parallel plates; tripod staff; in which the telescope tube is packed; repeating table; level collimator, with micrometer eyepiece; and Troughton's levelling staff.

15. Level collimator, plain diaphragm.

16. 10-inch reflecting circle, by Troughton, with counterpoise stand; artificial horizon, with metallic roof; two tripod stands, one with table for artificial horizon.

17. Hassler's reflecting circle, by Troughton, with counterpoise stand.

18. 6-inch reflecting circle, by Troughton, with two counterpoise stands, one with artificial horizon.

19. 5-inch reflecting circle, by Lenoir.

20. Reflecting circle, by Jecker, of Paris.

21. Box sextant and 3-inch plane artificial horizon.

22. Prismatic compass.

23. Mountain barometer.

24. Prismatic compass.

25. 5-inch compass.

26. Dipping needle.

27. Intensity needle.

28. Ditto ditto.

29. Box of magnetic apparatus.

30. Hassler's reflecting circle, with artificial horizon roof.

31. Box sextant and $2\frac{1}{4}$-inch glass plane artificial horizon.

32. Plane speculum artificial horizon and stand.
33. $2\frac{1}{2}$-inch circular level horizon, by Dollond.
34. Artificial horizon roof and trough.
35. Set of drawing instruments, consisting of 6-inch circular protractor; common ditto; 2-foot plotting scale; two beam compasses and small T square.
36. A pentagraph.
37. A noddy.
38. A small Galilean telescope, with the object lens of rock-crystal.
39. Six levels, various.
40. 18-inch celestial globe.
41. Varley stand for telescope.
42. Thermometer.
43. Telescope, with the object-glass of rock crystal.

These are now in the apartments of the Society, with the exception of the following, which are lent, during the pleasure of the Council, to the several parties under mentioned, viz.:—

The *Beaufoy* transit, to the Observatory, Kingston, Canada.

The *Sheepshanks* instrument,		No. 1, to Mr. Lassell.
Ditto	ditto	No. 2, to Mr. De La Rue.
Ditto	ditto	No. 3, to Rev. F. Howlett.
Ditto	ditto	No. 4, to Rev. C. Lowndes.
Ditto	ditto	No. 5, to Mr. Birt.
Ditto	ditto	No. 6, to Rev. J. Cape.
Ditto	ditto	No. 9, to Mr. Lockyer.
Ditto	ditto	No. 10, to Admiral Bethune.
Ditto	ditto	No. 41, to Rev. C. Pritchard.
Ditto	ditto	No. 43, to Mr. Huggins.

The 6-inch circular protractor, to Mr. Birt.

The Council cannot recollect any former occasion on which there has been better ground for congratulation to the Royal Astronomical Society than at the close of the past year. Looking backwards ten years, they find the number of the contributing members has increased by nearly thirty per cent. The attendance at the Evening Meetings has more than doubled, and the funded property of the Society, during the nine years' tenure of office by the present Treasurer, has increased by upwards of £2700 Stock. Applications for the supply of the *Monthly Notices* of our proceedings continue to be made from every quarter of the globe; and several of the numerous private Observatories scattered throughout the country are showing signs of increasing vitality by the production of fresh and valuable results.

Time, however, as upon all else, so upon us, has left its

inevitable mark; and a Society which celebrates, as we do to-day, its forty-sixth anniversary, can scarcely hope to find many of the cherished names of its original founders still remaining on its lists. Of those eminent and energetic men who met together as the germ of the Royal Astronomical Society, on the 12th of January, 1820, *three* only now survive; but it is worthy of our most grateful observation, that in Mr. Babbage, Sir John Herschel, and Sir James South (for these are the three), we recognise names which are among the most honoured that have adorned our annals.

The Volume of *Memoirs* will shortly appear. It contains three Papers, these are, first, a Memoir by Mr. Stone, the First Assistant in the Royal Observatory, Greenwich; the title is "On the Accuracy of the Fundamental Right Ascensions of the Greenwich Seven-year Catalogue for 1860."

The second Memoir, also by Mr. Stone, is "Constant of Lunar Parallax."

The third Memoir, "On some peculiar instances of Personal Equation in Zenith Distance Observations," is by Mr. Dunkin, also of the Royal Observatory.

To these Memoirs are appended three communications from Sir Thomas Maclear, entitled—

1. "Geocentric North Polar Distances of the Moon and Moon-culminating Stars from Observations made with the Transit Circle at the Royal Observatory, Cape of Good Hope."

2. "Right Ascensions and North Polar Distances of Comet I., 1865, derived from Observations made with the $8\frac{1}{2}$ feet Equatorial."

3. "Mean Right Ascensions and North Polar Distances of Comet I., 1864, derived from Observations made at the Royal Observatory, Cape of Good Hope, by W. Mann, Esq. First Assistant."

The expense of printing these last Papers is defrayed by the Government.

Obituary.

During the past year the Society has to regret the loss by death of more than an average number of long-valued and distinguished Members, some of whom will be greatly missed, not alone by our own body, but by the whole commonwealth of Science. They are as follows:—

Honorary.

The Duke of Northumberland.

Ordinary Members.

Dr. Burder.
S. H. Christie, Esq.
Benjamin Gompertz, Esq.
J. W. Grant, Esq.
Sir William Rowan Hamilton.
Rev. John Hind.
Sir John Lubbock, Bart.
F. W. Simms, Esq.
Vice-Admiral Smyth.

Associates.

Prof. G. P. Bond, of Cambridge, U.S.
Prof. Encke, of Berlin.

William Corbett Burder was born at Stroud, Gloucestershire, October 30th, 1822. His father was the Rev. John Burder, M.A., who survives him, and his grandfather was the Rev. George Burder, the author of *Village Sermons*. After serving five years' apprenticeship to an architect in Bristol, he left that profession, and, removing to London, studied architectural engraving under Mr. J. H. Le Keux. His success in this pursuit is shown by the engagement which Mr. Le Keux afterwards made with him for his services, as well as by the published portion of an unfinished work entitled *Architectural Antiquities of Bristol*, the illustrations of which he both drew and engraved.

As a meteorologist he was widely known from his long connexion with Mr. Glaisher's corps of observers, and by the notices which he frequently sent to the *Times* on matters of meteorological interest. He also published a few years since a little volume, the *Meteorology of Clifton*, containing the condensed results of ten years' observations at that place. A severely accurate and conscientious observer himself, he instinctively detected the want of these qualities in others, and was always displeased when he found that a scientific statement had been coloured to serve a purpose, or a phenomenon recorded with a show of precision beyond what the circumstances permitted. At the root of this feeling was, doubtless, the intense love of truth which distinguished him through life, and was indeed the dominant principle of his character. To the same cause may be traced the antipathy which he entertained to the whole race of weather prophets, a class of persons whom he disliked the more, as they tended to bring meteorology itself into disrepute among the half-informed.

In Astronomy, Mr. Burder found a source of genuine pleasure. A trivial incident shows how early the bent of his mind in this direction declared itself. A lady who kept a preparatory school at which he was a pupil when about seven years of age relates, that on one occasion he insisted that the

play-hour, 12 o'clock, had arrived, because he had observed that at that hour the shadows of the window-bars took a particular position on the floor,—a very juvenile attempt at a meridian observation, and it is believed self suggested. He was ingenious in the construction of instruments, and devised a portable equatoreal, of which he published an account in *Recreative Science.* He also constructed a clock, the accurate performance of which surprised and pleased him. He often employed spare moments in the apparently unprofitable task of scouring the heavens with the naked eye for new comets, and it is remarkable that on two occasions his search was rewarded. The particulars of these two discoveries (made on March 28, 1854, and June 30, 1861), are related by the Hon. Mrs. Ward in *Recreative Science* for August 1861. His sight was very acute, as was also his ear. He had a good knowledge of music, and played agreeably on the flute and violin.

For the last two years of his life increasing weakness confined him almost entirely to his room, and during this period his mechanical ingenuity showed itself in a great variety of contrivances adapted to meet his special wants. One of these was a mirror, so placed as to reflect into his room the image of a vane erected on the roof of the house. Another was a ventilating apparatus, constructed so as to convey a constant supply of fresh external air, brought up to the temperature of the apartment by passing through a certain length of tube, and discharged at a point vertically over his face as he lay in bed. His condition of chest rendering him acutely sensitive both to cold and impurity of air, he set great value on this invention, and was led into an extensive correspondence with persons whose attention had been attracted by his description of it in the *Times.*

A rather sudden accession of unfavourable symptoms occurred about six weeks before his death, and from this time no prospect remained beyond a temporary rally. Yet even through this period he retained an interest in his former pursuits, and his love of nature never failed him. One evening during this time, being told of a beautiful sunset, he requested a mirror to be placed so as to reflect it, and by this means was enabled to see it as he lay in bed. His sufferings were at intervals severe, but his mental faculties remained unclouded, and his last moments were eminently tranquil and happy. He died on the 16th of October, 1865, at the age of 43, devotedly beloved by those who stood nearest to him, and sincerely lamented by a wide circle of friends.

Benjamin Gompertz, a member of the Jewish community, was descended from a family that long held a distinguished position in Holland. His grandfather on the mother's side, Benjamin Cohen by name, was on intimate terms with William

Prince of Orange; and it is related that during the troubled times of the first French Revolution the Dutch Stadtholder found a ready asylum at the magnificent mansion erected by his friend at Amersfort. Benjamin Cohen was a man of intellectual tastes, evinced, among other ways, by his causing a translation of Euclid into Hebrew to be made for his use. The father of Benjamin Gompertz was a successful diamond-merchant, a pursuit for which the Dutch appear to possess a singular aptitude even to the present day. The literary tastes of the grandfather descended to the grandchildren, and all the brothers of Benjamin Gompertz became, like himself, men of mark in their generation. His brother Isaac wrote poems, possessing a merit sufficient to attract the approbation of Byron. Ephraim Gompertz, still living, is known among a large circle of acquaintance to be an accomplished and original mathematician. Lewis Gompertz, the youngest brother, was the founder of the "Animals' Friend Society," and he entertained and published many a strange speculation on the relation of the brute creation to man.

Benjamin Gompertz, the subject of this notice, was born on the 5th of March, 1779, at Bury Street in the City of London. He did not enjoy the advantage of a collegiate education, but upon the whole must be considered as a self-taught man, and to this circumstance, perhaps, may be attributed some of the originality of pursuit and of conception for which he was remarkable. He became familiar with the writings of the French mathematicians of the last century, but his favourite authors were Emerson and Maclaurin, and beyond all others Newton. So great an admirer was he of the latter immortal philosopher, that to the last he retained his predilection for the "Fluxional Calculus," not only with respect to the spirit of its conception, but even for the symbols of its notation. The form of his mind seems to have been eminently truthful, and hence, one among other reasons which he assigned for his rejection of the differential notation of Leibnitz was, that it was "furtive."* In this paper, Mr. Gompertz also proposes what he considered to be certain improvements in the notation of partial fluxions or differential coefficients, and in the symbols for denoting logarithms and antilogarithms; but especially one for the embodiment of zeros which may fairly be considered as a great boon. Thus, according to Mr. Gompertz, 898,000,000 or 000,000,898 may be written with great convenience $898^{(6)}$ and $^{(6)}898$ respectively.

Mr. Gompertz while still a youth, in fact as early as the year 1798, took a prominent part among the English mathematicians of the day. He was a constant contributor to the

* "I call the differential notation furtive," says he in the *Phil. Trans.* for 1862, "on, I think, a moral ground. The moral ground is, that it appears to give to Leibnitz a greater claim to originality, to the prejudice of Newton, than I think he is justly entitled to."

ingenious questions contained in the *Gentleman's Mathematical Companion;* and the extent of his analytical powers was fully proved by the fact, that although such men as Ivory, Nicholson, Griffith, Davies, and others, were constant contributors, nevertheless, from the year 1812 to 1822, and without a single exception, Benjamin Gompertz carried off the prize annually offered for the best solution of the prize question.

Young Gompertz was originally intended by his father to follow a purely mercantile profession, and with this view he made his first start in life on the Stock Exchange, and continued a member of that body almost to the time that he undertook the management of an Insurance Office.

In 1810 he married Miss Abigail Montefiore, the sister of the well-known philanthropist, Sir Moses Montefiore. Two daughters still survive this union. The din of Change Alley was but little congenial to the mind of Benjamin Gompertz, and, like his eminent friend Francis Baily, he was only too glad when the hour came that he could retire to the quiet of his own study, or to the instructive discussions of the various learned societies of which he became himself an active member. It was here that he rejoiced to cultivate a friendly interchange of thought with such men as Sir John Herschel, Sir James South, Mr. Babbage, Mr. De Morgan, and many other well-known names in the household of science.

The first learned society that Mr. Gompertz joined was the "Mathematical," whose members met at Crispin Street, Spitalfields. The object of this society, founded so far back as 1717, was to assist the middle classes in their cultivation of mathematical pursuits, by affording access to books on scientific subjects which at that day were accessible to the opulent alone. In the operations of this valuable society, Mr. Gompertz took a prominent part, and his great abilities and constant urbanity there, gained for him so much good will, that eventually he became President of the society, which office he retained until the members amalgamated themselves with the Royal Astronomical Society, and transferred to us their valuable collection of books and instruments.

In 1806 Mr. Gompertz contributed his first original paper of importance to the Royal Society, through Dr. Maskelyne, the Astronomer Royal of that day. This memoir treats of the summation of certain series by the method of differences, which theretofore had been summed by Landen through the aid of imaginary quantities. It was possibly the composition of this memoir which induced Mr. Gompertz to direct his attention to the interpretation and calculus of imaginary symbols, which soon occupied his thoughts for a considerable period. The results of these speculations he published in a series of tracts at his own expense, and next to his mathematical expression for the law of mortality, he even considered them as the most successful of his essays.

In 1819 Mr. Gompertz was elected a Fellow of the Royal

Society, and for forty-six years he continued an active member of that illustrious body, and occasionally served as a member of its Council.

In the year 1820 occurs the foundation of our own Society, and if Mr. Gompertz cannot be with strictness considered as one of its actual founders, it is certain that he became one of its warmest and most active supporters from the day of its foundation. On Feb. 9, 1821, he was elected on the Council, and of it he continued a most valuable member for nearly ten years. Many of the most valuable contributions to the *Memoirs* of our Society were committed to his care, and not a few thus entrusted to him were elucidated and rendered more complete by his explanatory notes. Instances of this will be found in Littrow's paper "On the Transit Instruments," (*Astron. Trans.* vol. i. p. 275), and in Kreil's paper "On the Equatorial," vol. iv. p. 501.

The first contribution made by Mr. Gompertz to our *Memoirs* was in 1822, on the subject of "The Aberration of Light;" and in this memoir occurs a passage which it may not be inopportune here to transcribe, not alone for the truth which it contains, but for the sake of the picture which it presents to us of the enthusiasm with which Gompertz was animated in his pursuit of science. "In the contemplation of the sciences," he says, "there is, besides the pleasure arising from the acquirement of knowledge of practical utility, a peculiar charm bestowed by the reasoning faculty itself in a well-directed pursuit of facts; and though the results deduced by the arguments are frequently considered to be the only objects of value by the unlearned, the man of absolute scientific ardour will often, while he is enraptured with the argument, have not the slightest interest for the object for which his argument was instituted." This remarkable evolution of Mr. Gompertz's mind may probably serve as the clue explaining the fact that, although he enriched the *Memoirs of the Royal Astronomical Society* with many admirable contributions on the subject of the corrections of astronomical instruments, he in no other sense ever became a practical astronomer, or was much habituated to the use of the instruments, with the construction and errors of which he was thus manifestly familiar. Thus we have in the first volume of our *Memoirs* (Nov.) papers and a supplement on the "Theory of Astronomical Instruments." Again, in the volume for 1824, there occurs the description of a new astronomical instrument, named by the late Francis Baily "The Differential Sextant." There is also a contribution on the subject of the corrections due to errors in the knife-edges of Kater's Convertible Pendulum; but the most practically important astronomical object to which Mr. Gompertz directed his attention was that in connexion with formulæ for the reduction of the apparent places of the stars to their mean co-ordinates. What Mr. Gompertz

effected in this direction may probably be most easily understood from the following quotation of words spoken by Sir John Herschel in his well-known *éloge* of the late Francis Baily : "It seems almost astonishing that these computations, which lie at the root of all astronomy, and without which no result can be arrived at, and no practical observer can advance a single step, should have remained up to so late a period as the twentieth year of the nineteenth century in the loose and troublesome state which was actually the case, and that not from their theory being not understood, but from their practice not having been systematized. . . . Messrs. Baily and Gompertz, perceiving this want, proceeded to supply it. The subject was investigated generally, and a method was devised for arranging the terms of the corrections for Aberration and Solar and Lunar Nutations. Some of the tables had already been computed, when they heard of Bessel's labours in the same field. Finding that that astronomer had proceeded on a similar principle, but that besides the other corrections he had taken in that of Precession, Messrs. Baily and Gompertz willingly gave way, and certainly nothing more perfect than the 'Elementa Astronomica,' by Bessel, could have been devised. However, Messrs. Baily and Gompertz were of some use. The complete Catalogue of the Royal Astronomical Society may be partly looked upon as the fruit of the united labours of these two men."

Such, then, are the labours of our late most excellent member in that field of science which we especially cultivate. With the exception, indeed, of certain investigations, unfortunately left incomplete, on meteors and shooting-stars, which occupied his attention during a part of the last years of his life, his most recent contribution to our *Memoirs* is dated so far back as the year 1829.

It does not seem to be the province of our Society to record, more than by way of the most general reference the labours of Mr. Gompertz in that branch of science in which, perhaps, no man has ever been more highly distinguished We allude to his admirable and most valuable contributions to the theory of Life Assurances and Contingent Reversions, which never ceased to occupy his thought during the remainder of a long and valuable life. The name of Gompertz must be fresh in the recollection of every actuary who truly studies his own profession; and it may not be too much to say that his writings will ever form one of the landmarks in a science which silently but effectually intertwines itself into the material interests of society.

It is thus that, in a brief paragraph, we simply, as members of an astronomical society, pass over a long period of activity replete with interest to other men whose tastes and pursuits are attracted or determined by the interest they take in all that concerns the statistics of human life. In other and

more fitting publications they will find the records of these labours of this remarkable man. To us it must be of interest to know that, respected by those who saw him only at a distance, or through the medium of his works, and beloved by his family and those who saw him in the closer relations of life, Benjamin Gompertz, at a ripe old age, died on July 15, 1865, and was buried in the Jewish Cemetery, near Victoria Park.

William Rowan Hamilton, one of the ablest mathematicians that this or any other country has produced, and for nearly forty years a Fellow of the Royal Astronomical Society, was born in Dominick Street, Dublin, in the year 1805. His father was by profession an attorney, and was long held in great estimation both for his personal character and his professional ability. The branch of the Hamilton family from which he was descended, originally settled in the north of Ireland, in the reign of James the First; and it is said that by right a baronetcy belonged to the representative of this branch, a near relative of his own; although the claim could not be fully supported, owing to merely technical flaws. Thus Hamilton may have been in some degree indebted for his great and versatile mental capacity to a mixture of race.

William Hamilton is one of those rare instances, where the promise of early childish precocity has not been disappointed by the attenuated achievements of riper years. At various stages of his boyhood, not to say childhood, for the precocity manifested itself at the early age of *four*, he is said to have successively acquired some notable acquaintance with no less than thirteen languages, European and Asiatic. His attention was directed to the latter, because it was originally hoped that, enjoying as he did the opportunity of good patronage, his career would be passed in India. It is recorded on evidence which deserves respect, that at the age of seven he was examined in Hebrew by a fellow of Trinity College, Dublin, and that "the child passed a better examination in that language than many candidates for the fellowship." For obvious reasons we hope there is some pardonable though very natural exaggeration in the statement. It is certain however that the attention of the Persian Ambassador, when on a visit to Dublin, was attracted by a letter of greeting written in Persian by young Hamilton at the age of fourteen. Whether or not any allowance is to be made for the shadow of the future overlapping the memory of the past, it is quite certain that the vast intellectual capacities of the boy were evinced and cultivated at a very early age, and what is of far greater consequence, this early mental activity did not prostrate or forestal the successful exertions of maturer life. It is quite possible that the literary turn thus given to his earlier pursuits may have happily laid the foundation of that peculiar combination of metaphysics and poetry, which distinguished

some of his mathematical performances from those of most other men. For his early training in ancient and modern languages, he was indebted to the loyal care of his uncle, the Rev. James Hamilton, curate of Trim; but in science and mathematics he appears to have been nearly self-taught and self-directed; in his case, as in that of many other eminent men, this circumstance probably conduced to the originality of his maturer conceptions, and to the peculiar style in which he embodied them.

By the age of fifteen, young Hamilton had mastered the usual course of elementary mathematics, pure and applied; and in some instances had become familiar with works of original research. He appears to have evinced a peculiar taste for long and difficult arithmetical approximations, and to have shown himself no mean antagonist in the solution of numerical puzzles when matched against a certain arithmetical prodigy, who, coming from America, happened at that time to be exhibited in Dublin. By the age of seventeen, he had mastered Newton's *Principia*, and a year later found him in possession of most of the processes in the *Mécanique Céleste*. Meanwhile, and notwithstanding this very unusual advancement in mathematical knowledge, the main culture of his mind had been classical; and that, not alone from natural predilection, but on account of the requirements of the collegiate course on which it was his intention to embark and to compete.

It is almost needless to say that young Hamilton, with a mind thus disciplined and furnished, entered upon his course at Trinity College, Dublin, if not without able competitors, at all events without an equal, whether in literature or mathematics. As might be expected, he carried all before him; and when we speak of success in his literary efforts, it must be understood that we include Poetry in the list, inasmuch as on two successive occasions he gained the Vice-Chancellor's Prize for English verse. It is to this early and successful cultivation of the lighter elegancies of scholarship that his friends were indebted for a vein of poetical thought and expression which graced alike his correspondence and his conversation, and which is sometimes observable even in his graver compositions.

It appears that in the year 1822, one year before his entrance at the University, young Hamilton, now in his eighteenth year, attracted the notice of the celebrated Dr. Brinkley by certain objections which he made to a demonstration propounded by Laplace in the *Mécanique Céleste*. On being invited to pay a visit to that well-known astronomer, the young student thought that he should most properly express his feelings of respect by carrying in his hands another instance of independent research on the osculation of certain curves of double curvature. This introduction of Hamilton to the veteran professor laid the foundation of a mutual friendship

and respect which continued to increase during Dr. Brinkley's tenure of office.

In the first year of his student life at Dublin, Hamilton, notwithstanding his close attention to the elementary line of study necessarily prescribed to under-graduates, nevertheless engaged himself in a line of original research. Even before his entrance at the University he had directed his thoughts to the difficult subject of Caustics, and having now completed the memoir, it was read before the Royal Irish Academy in 1824. This paper was referred as usual to the consideration of a committee of scientific men, who, being struck with the originality of the conception, and the evidences of analytical power which it contained, recommended the author to give those further developments of the subject which evidently lay within his grasp. The result of this encouragement to the young philosopher was the speedy completion of a memoir which may be said to contain the germ of a large portion of the noble work which it was his lot to contribute towards the advancement of physical knowledge. Instead of an essay on Caustics, his paper was now enlarged into a wider and more general investigation, under the title of a "Theory of Systems of Rays." It may be no exaggeration to say of this memoir, in conjunction with its subsequent supplements, that it is one of the ablest contributions ever made to our knowledge of the geometry of optics. Chasles, one of the most distinguished of modern geometers, speaks of it as "*dominant toute cette vaste théorie.*" Starting from the simple fundamental principle that light, whatever may be its cause or its constitution, is amenable to what mathematicians call "The Principle of Least Action," or, in other words, probably as true, and certainly more expressive, amenable to the *principle of no waste in nature*, Hamilton, in a train of analytical logic unimpeachable, and with a mastery over the management of algebraic symbols probably never surpassed, shows that the theory of a system of rays reflected or refracted any number of times at given surfaces, depends on the determination of a single principal function V, which contains in itself all the properties of the system of rays, in a manner analogous to that in which the properties of a curve are contained in its equation. The same theory is, in the supplements, extended to the more complicated and recondite question of double refraction in biaxal crystals, and at length lands the reader in one of the most remarkable scientific predictions contained in the records of physical inquiry. But of this prediction we must speak presently.

When the first part of this "Theory of Systems of Rays" was presented in April 1827 to the Royal Irish Academy, it will be remembered that Hamilton was as yet an undergraduate of twenty-one years of age. In this year the Professorship of Astronomy in Trinity College, Dublin, became vacant by the promotion of Dr. Brinkley to the bishopric of Cloyne. Such

was his deserved reputation, that, notwithstanding the appearance of other and most formidable candidates in the field, and although, moreover, he had as yet taken no academical degree, Hamilton was elected to the vacant chair.

This circumstance is of itself sufficiently remarkable, and reflects equal honour upon the authorities who ventured to make the appointment, and on the young geometer who, by dint of genius and laborious study, was qualified to discharge the duties of the post. In connexion with this arrangement there is a point of osculation with our own Society of sufficient interest to demand our notice. The present Astronomer Royal, at that time Lucasian Professor of Mathematics in the University of Cambridge, was one of the candidates for the vacant post at Dublin; and he too, like Hamilton, had been advanced to his professorship before he had ceased nominally to be in pupilage. We are not here, even by the remotest implication, suggesting a comparison between these eminent men; such a comparison would not only be utterly unfitting, but, owing to the divergence of the lines of research adopted by the two Geometers, would be wholly impossible. Nevertheless the thought unavoidably presents itself, that for both parties, and for the general interests of science, the decision of the Dublin electors was a happy one. Had it been otherwise, the one, in all probability, from certain natural tendencies of his mind, would have become a clergyman — no doubt a most eminent one — in the Irish Church; while Greenwich and our own Society might have lost the other.

> "There is a Divinity which shapes our ends,
> Rough-hew them how we will."

In 1828 Hamilton became a Fellow of the Royal Astronomical Society, and thus at the time of his decease was among the oldest, as his name was certainly among the most honoured, of our members. In 1833 he made known, in one of several supplements to the "Theory of Systems of Rays," his great discovery of Conical Refraction. In this memoir, starting again from the principle of least action, and, as before, conducting the investigation by means of a single Principal Function, he establishes the entire theory of double refraction; and, applying it to the case of biaxal crystals, by a new and simpler method* than that originally pursued by Fresnel, he obtains the equation to the form of the wave assumed by the vibrating ether within the crystal. On examining the form of the wave surface, Hamilton, with remarkable sagacity, observed that if the theory and the results were true, a single ray of light incident at a certain angle on a biaxal crystal, must of necessity pass into it, not as one ray,

* It is but a point of justice to state that Mr. Archibald Smith has subsequently much improved the simplicity of the process by a very elegant method of elimination.

nor even as two rays, but as a conical sheet of light, and then finally emerge as a luminous cylindrical surface. And, again, his profound and complicated analysis indicated that there was also a direction within the crystal, such, that if an internal ray of light passed along it, it would emerge from the crystal, not as one ray, but as a luminous conical shell. Such results as these were not only apparently contrary to all analogy and expectation, but formed, if the experiment could indeed be made, a species of *experimentum crucis* of the truth of the undulatory theory of light. Notwithstanding the difficulty of the case, the experiment was at length successfully performed by Dr. Humphrey Lloyd, of Dublin, whose patient ingenuity, and faith in the profound work of the geometer, were rewarded by the sight, for the first time, of what cannot properly be called less than the astonishing phenomenon of a single ray spread out, by refraction in a crystal, into an infinite number of rays, forming the surface of a luminous cone.

From the sagacity of Hamilton, and of his friend Dr. Lloyd, thus constraining the little crystal of Arragonite to give up, Sphinx-like, its secret of ages, our thoughts unavoidably turn to the parallel case of Adams and Leverrier, who, from a similar strong faith in the laws of nature and in the logic of geometry, not only predicted the existence of a planet heretofore unseen and unexpected, but indicated the precise region of the heavens, where, as soon as looked for, it was actually found. We do not regard such results as valuable only because they corroborate our conviction of the existence of certain laws whereon we believe the universe to have been constructed by the Author of Nature, but still more so because they serve to encourage the student to persevere in his researches, animated by the fullest conviction that if truthfully conducted they can only land him in truth, and leaving the *cui bono* to be determined by the appreciations, or the wants, or the curiosities, of men in time to come.

The Royal Irish Academy took cognisance of Hamilton's great discovery, and of the profound mathematical skill whereby it was evolved, by conferring upon him their Cuninghame medal; and the Royal Society awarded to him a similar mark of their appreciation of his merits. In 1837 he was elected President of the Royal Irish Academy, succeeding* his friend and early patron, Dr. Brinkley, in the chair, as he had succeeded him in the Professorship of Astronomy. He retained this distinguished office for eight years, and on his resignation he received the thanks of that eminent Academy "for his high and impartial bearing in the chair."

In 1834 and 1835 he communicated to the Royal Society two papers on "A General Method in Dynamics." Here,

* Dr. Lloyd, sen., was President for two years after the death of the Bishop of Cloyne. Hamilton succeeded Lloyd.

again, he commenced with the same fundamental idea, as that which he had already so successfully adopted in his "Theory of Systems of Rays," and he showed that the integration of the differential equations of motion for any system of bodies may be considered as depending on the determination of a certain Principal Function, which he defines in several different forms, but in each case by means of *two* partial differential equations involving, one of them, the differential coefficients in regard to the final co-ordinates (co-ordinates at the time t), the other, those in regard to the initial co-ordinates of the several particles. He also established in these Memoirs the now well-known "*Hamiltonian Form*" of the equations of motion of any material system.

The two Memoirs just referred to gave occasion to Jacobi's investigations on "Partial Differential Equations" (*Crelle*, t. xvii. 1837). Jacobi shows that, instead of "Hamilton's Function" involving the time and the initial and final co-ordinates, and satisfying *two* partial differential equations, it is allowable to consider a function of the time and the final co-ordinates only, satisfying a *single* partial differential equation; and he considers that by omitting to make this simplification, Hamilton presented his remarkable discovery in at least an imperfect light. There can be no doubt that the simplification thus introduced by Jacobi was a most important and valuable one; but it can scarcely be objected to Hamilton that he failed to perceive all the results deducible from his own discovery, any more than it can be objected to Fresnel that he left it to Hamilton to deduce conical refraction from the very form of the wave surface which Fresnel was the first to investigate. It must not be forgotten that it is to Hamilton's discovery as their fountain, though the course of the stream was directed by Jacobi, that are due all the developments which have since been made in the vast subject of Theoretical Dynamics. In a word, it may not be too much to say that the step in advance made by Hamilton's two memoirs can only be compared with that effected at an earlier epoch by the publication of Lagrange's *Mécanique Analytique*. For this work, also, Hamilton was again awarded a gold medal by the Royal Society.

We pass over various other characteristic works of this profound analyst, not because they are devoid of interest or of worth, but because they are less within the scope of our Society; and we come at length to what Hamilton considered the crowning labour of his life,—a labour which for the next twenty years, and indeed till within a few days of his decease, continued to occupy his thoughts. The labour here referred to was bestowed on the invention and the development of the Calculus of Quaternions. In a memoir such as this, and for the purposes which we have in view, we must almost despair of explaining, or perhaps of even conveying an

idea of what is the aim and scope of the Calculus of Quaternions, or in fact what a Quaternion is, and yet without some such attempt, successful or not, any obituary notice of this great man would be incomplete. For this purpose, then, we must bear in mind that in the method of geometry introduced by Descartes, and which has been retained in astronomical and physical investigations up to the present time, the position of a point in space has been determined either by its distances from three co-ordinate planes, or by what in reality are their equivalents. Hamilton, however, starts at once by considering not so much the position of a *point*, as rather the relation which exists between two *lines* intersecting in space, having regard both to length and to position. It will soon be seen that in order to determine these relations completely, four quantities, or four elements, are necessarily involved.

1°. There is the relation which the length of the one line bears to the length of the other line;

2°. The angle through which the one line must be conceived to be turned in order that it may coincide with the direction of the other;

3°. The plane in which the two lines lie.

And inasmuch as the determination of this plane involves two elements, viz., 1°. its inclination to some fixed or known plane, and 2°. an element which is analogous to the longitude of a planet's node, it follows that four* elements or symbols are required to determine the relation which one line in space bears to another line.† The combination of these four elements, then, forms the Quaternion of Sir William Hamilton; and as handled and developed by him, these combinations unquestionably form a calculus of amazing generality, grasp, and power. As an engine of investigation, in the general problem of

* The above is in fact one of Hamilton's many illustrations of the meaning of a quaternion. Analytically speaking, a quaternion is an expression of the form $w + ix + jy + kz$, where i, j, k are imaginary roots of $\sqrt{-1}$, differing from the imaginaries of ordinary algebra, in that the *order* of multiplication of these symbols is material, ij here not being $= ji$ but $= -ji$, and so for the other symbols.

The geometrical interpretation is this: on taking the usual three rectangular co-ordinate axes of x, y, z; if ij means rotate the axis of (y) round the axis of (x) through 90° of right-handed rotation, then ji must mean rotate the axis of (x) round the axis of (y) through 90° of right-handed rotation. Now the result of the former rotation is a line in the direction of the axis of $+z$; the result of the latter rotation is a line in the direction of the axis of $-z$: in this sense then $ij = -ji$ and so $jk = -kj$, and $ik = -ki$. The symbol (w) is the ratio of the lengths of two intersecting lines (or *vectors*) considered in the quaternion. Such is a first glimpse of this intricate Calculus.

† *Elements of Quaternions*, Longmans, 1866, page 110. This extraordinary work is the result of the unceasing labours of the two last years of Sir William Hamilton's life: indeed, it is said to have been fatally injurious to his health. It was all but finished when the lamented death of the author arrested its entire completion. The Board of Trinity College, Dublin, have marked their sense of the value of this book by defraying the expenses of its publication.

combined rotations, the method of Quaternions probably has no rival in completeness or in facility. They remind one of the tentacles of some gigantic polype, ramifying out into immensity, and bringing back with them the spoils of space.*

It is as yet premature to anticipate on which of his investigations or discoveries Hamilton's fame will ultimately rest. There are mathematicians among us who in this respect would be inclined to name his Calculus of Quaternions; others would say that none of his writings can overshadow the importance of his *Dynamical Theorems*. As yet, however, the former Calculus can hardly be said to be fully developed, or to have been extensively applied by other philosophers to new lines of investigation; nevertheless, it can scarcely be supposed that the persistent and conscientious labour of such a man for twenty-two successive years can fail to be full of the seeds of thought, and one day be found to admit and to invite important applications. It must however be conceded that (partly perhaps on account of its comparative novelty, and partly on account of the metaphysical atmosphere which surrounds it), the method is neither easy nor attractive to any but the ablest and most daring of the analysts among us; many a man who has essayed to bend this bow has probably said to himself what Antinous said to his boon companions:—

> "Thou wast not born to bend
> The unpliant bow, or to direct the shaft."†

We have just spoken of the metaphysical atmosphere which seems to pervade Hamilton's Calculus of Quaternions; and herein there is little to excite our surprise, for it was natural for a man possessed of a mind so versatile and so profound, to turn it inwards on itself; hence he delighted in metaphysics. But it was not alone because the culture and bias of his mind unavoidably led him in this direction, that many of his mathematical investigations assumed a metaphysical turn, but because he, in conjunction with other thoughtful philosophers, believed that no further great advance in mathematical science was now to be expected, excepting from the metaphysical point of view. Probably it is either a conscious conviction, or an intuitive perception of this, which influences the peculiar phase observable in the mathematical investigations of some of our greatest analysts of the present day.

Hamilton was not only a great mathematician, but by nature he was also a poet. He was heard to say, "I *live* by mathematics, but I *am* a poet." If, by this aphorism, he meant that, had he so chosen, he would have become more eminent as a poet than he is as an analyst, bystanders might hesitate to give their assent. Few men, perhaps, are fully

* With this simile Sir W. Hamilton expressed his acquiescence to the writer of this memoir.

† Cowper's translation of the *Odyssey*, book xxi.

conscious of the ruling bias and the strong points of their own minds. We know one of our greatest living philosophers who would perhaps say, "By filial duty I am an astronomer, but I was born a chemist." Of another it has been often said, "He *is* a mathematician and an observer, but he *was born* an engineer." Nevertheless Hamilton was a true poet, and by no means an indifferent writer of true poetry; and it is quite certain, that some of our subtlest mathematicians are poets at heart, knowing it and feeling it. And here it may be worth a passing remark to mention that Hamilton, in his great Memoir on *A General Method in Dynamics*, speaks of Lagrange's *Mécanique Analytique*, as a *Poem*. One of our chief living astronomers hereon remarks: "Hamilton was right, but he might have said a poem of most stately rhythm." The two works of Lagrange and Hamilton have points in common.

Hamilton counted among his friends, Coleridge, Southey, Mrs. Hemans, and Wordsworth. It is said, that when the latter, through Hamilton's enthusiasm, was enabled to get a glimpse of the inexpressible fascination which surrounds the daring and *creative* spirit of modern geometry, the old man was for the first time inclined to admit even a mathematician into the charmed circle of the brotherhood of poets. The anecdote rests upon unquestionable authority: nevertheless, we are inclined to think better things of so great and profound a mind as that of Wordsworth, and we are convinced that he must, by sheer dint of sympathy with other minds, have had at least a suspicion of the fact before the great analyst revealed it. In vindication of the justness of these remarks on the expansiveness of great intellects, and on the poetic power which almost invariably is, at the least, latent within them, we cannot refrain from quoting the following Sonnet, written by a great Astronomer, on the occasion of a visit to Ely Cathedral, in company with Sir William Hamilton:—

Sunday, July 29, 1845.

The organ's swell was hushed,—but soft and low
An echo more than music rang,—where he,
The doubly-gifted, poured forth whisp'ringly,
High-wrought and rich, his heart's exuberant flow,
Beneath that vast and vaulted canopy.
Plunging anon into the fathomless sea
Of thought, he dived where rarer treasures grow,
Gems of an unsunned warmth, and deeper glow.

Oh! born for either sphere, whose soul can thrill
With all that Pöesy has soft or bright,
Or wield the sceptre of the sage at will,
(That mighty mace * which bursts its way to light),
Soar as thou wilt, or plunge,—thy ardent mind
Darts on—but cannot leave our love behind.

* The symbolic analysis of which the eminent and excellent individual (Sir W. R. H.) supposed to be addressed has proved himself a most consummate master.—(*Essays by Sir John Herschel.*)

This Memoir would be incomplete if we did not add, that our deceased member, together with the character of a scholar, a poet, a metaphysician, and a great analyst, combined that of a kind-hearted, simple-minded Christian gentleman; we say the latter because Sir William Hamilton was too sincere a man ever to disguise, though too diffident to obtrude, his profound conviction of the truth of revealed religion. Endued with such qualities as these, what wonder, if of his friends he was almost the idol, and of his university the pride; for he was gentle, and he was eloquent, and he spoke evil of no man, he defended the fair fame of the absent, and he held controversy with none.

Such, then, is an imperfect but unexaggerated sketch of this remarkable man. We will only add, that happily he did not live to survive himself, but in full possession of his faculties, almost in the very presence of the friends who had long admired him; and, what was no new thing to him, supported by the convictions and consolations of his faith, he resigned himself to his rest, as one who knew that he had done a work which had been given him to do. [C. P.]*

The late Sir John William Lubbock, Bart., born in 1803, was educated at Eton, and at Trinity College, Cambridge, where he graduated in 1825, being first in the Senior Optimes.

He was many years Treasurer and Vice-president of the Royal Society, and Vice-chancellor of the University of London; was a member of most of the principal scientific societies in this country, of the American Academy of Arts and Sciences, and of the Academies of Turin and Palermo. He spent much labour upon the theory of the Moon, referred to in Grant's *History of Astronomy*, pp. 120–162. He also did much to improve the theory of the tides, for which he received one of the royal medals in 1833. The tides in the *Nautical Almanac* were for some time calculated from his tables (*Nautical Almanac*, 1848, part xi.).

In conjunction with Mr. Drinkwater Bethune he wrote an essay *On Probability* for the Society for the Diffusion of Useful Knowledge. This treatise, when reprinted, owing to some mistake of the binder, was described as "De Morgan on Probabilities." This extraordinary mistake was not discovered by Sir J. Lubbock until the work had been in circulation for some years; and Prof. De Morgan, in a letter to the *Times*, stated that "he could not, in fifteen years, though using every opportunity, succeed in restoring the book to its true authors."

* In the preparation of this *éloge*, the writer has received much assistance from Dean Graves, P.R.I.A.; the Rev. R. P. Graves, of Dublin; and Professors De Morgan and Cayley.

There can be no doubt that among all the mathematicians who have written upon the Lunar Theory, confessedly among the most difficult and most important questions in Physical Astronomy, Sir John Lubbock has a claim to a most conspicuous place. What he says of himself and of his coadjutors, in his Memoir published in the *Transactions of the Royal Astronomical Society* for 1860, is fully within the limits of justice and truth: "I am confident that a just posterity will give to us—that is, to Plana, Pontécoulant, and Lubbock, who in 1846 furnished the means of constructing tables of the Moon without any empirical hypothesis—the credit of first bringing the Lunar Tables within the limits of error of observation, and thereby of bringing to perfection the solution of the problem of finding the longitude at sea by means of lunar observations." Great, very great as is the merit of Sir John Lubbock in having thus brought the *theory* to perfection, it is much to be regretted that he did not take one step more, and that the comparatively easy step, of causing the Tables to be constructed.

Sir John Lubbock was one of the Treasurers of the Great Exhibition of 1851, and was a member of several commissions, among which we may particularly mention that for determining the standards, and also that appointed to investigate the question of weights and measures. He was for many years senior partner in his bank; and his last publication of importance was one on the *Clearing of the London Bankers*, to which he appended, as a motto, the well-known passage commencing,

> "Atque equidem, extremo nî jam sub fine laborum
> Vela traham, et terris festinem advertere proram."

He resided principally at his seat in Kent, where he took the greatest possible interest in the education of the neighbouring poor. He also devoted much time to agricultural pursuits, and was a successful breeder of stock, his southdowns and shorthorns having carried off many prizes. He married Harriett, daughter of Col. Hotham, by whom he leaves eleven children.

The accompanying list contains the titles of Sir J. W. Lubbock's principal publications:—

On the Theory of the Moon, and on the Perturbations of the Planets. Eight parts.

Account of the *Traité sur le Flux et Reflux de la Mer*, of Daniel Bernouilli.

An Elementary Treatise on the Computation of Eclipses and Occultations.

Remarks on the Classification of the Different Branches of Human Knowledge.

An Elementary Treatise on the Tides.

On the Heat of Vapours, and on Astronomical Refraction.

On Currency.

On the Determination of the Numerical Values of the Co-efficients in any Series consisting of Cosines of Multiples of a Variable Angle.

On Shooting-stars.

On the Attraction of Ellipsoids.

On Cask Guaging.

Note on the Calculation of the Distance of a Comet from the Earth.

On the Limits upon the Earth's Surface within which an Occultation of a Star or Planet by the Moon is Visible.

On the Divergence of the Numerical Co-efficients of certain Inequalities of Longitude in the Lunar Theory.

On the Census.

On some Elementary Applications of Abel's Theorem.

On the Double Achromatic Object-glass.

On some Problems in Analytical Geometry.

On a Property of the Conic Sections.

On a Property of the Conic Sections.

On the Wave Surface in the Theory of Double Refraction.

On the Variation of the Arbitrary Constants in Mechanical Problems.

On the Arabic Names of the Stars.

On the Stability of the Solar System.

On the Proceedings of the Excise Commissioners, with documents relating thereto.

Table of the Sines and Tangents (natural) for each degree of the Quadrant.

Table of the Logarithms of the Sines and Tangents for each degree of the Quadrant.

On the General Solution of Algebraical Equations.

On the Vapours of Ether, Alcohol, Petroleum, and Oil of Turpentine.

On the Conditions of the Atmosphere, and on the Calculation of Heights by the Barometer.

On Astronomical Refractions.

On the Gnomonic Projection of the Sphere.

Also, the Stars, in Six Maps.

In 1860 a most valuable paper, "On the Lunar Theory," in the *Memoirs of the Royal Astronomical Society.*

Frederick Walter Simms was born in London on the 24th December, 1803. Being in early youth of very delicate constitution, some difficulty was experienced in providing him with suitable employment, until, through the influence of his brother the late Mr. William Simms with Colonel Colby, he was despatched to Ireland as a civil assistant upon the Ordnance Survey. Active employment in the pure air of the mountains soon told favourably upon his health, and he returned to England with a tolerably vigorous frame.

Shortly after leaving Ireland Mr. F. W. Simms became an astronomical assistant at the Royal Observatory under Mr. Pond. In the year 1835 he resigned his situation, and returned to his former occupation as a surveyor and civil engineer. He visited France with Mr. Claridge, and joined with him in the introduction to this country of asphalte for pavements. In 1836 he engaged with the South-Eastern Railway Company as a resident engineer, a considerable portion of their works being under his direction. He constructed both the Bletchingley and the Saltwood Tunnels.

In 1846 the East India Company, having resolved upon the construction of railways in their territories, made a proposal to Mr. Simms for him to proceed to India as their consulting engineer, which, after a short delay, was accepted; but his health giving way from exposure to the climate, he obtained leave of absence for some time, and visited the Mauritius. Upon the reinstatement of his health he returned to his duty, and, amongst other work, he superintended a complete survey of Calcutta, which was executed principally by native assistants.

Having completed his engagements with the Company, Mr. Simms returned to England in the autumn of 1851, with a very shattered constitution, and although careful medical attention restored him to tolerable health, he lived in retirement afterwards, occupying himself partly by the education of his son. His death took place in Torrington Square, on the 27th Feb. 1865.

Mr. F. W. Simms was the author of several useful works in his profession, the principal being a *Treatise upon the Instruments employed in Surveying and Astronomy*; a *Treatise upon Levelling*; *Practical Tunnelling*. He also edited *The Public Works of Great Britain*.

Mr. Simms was a member of the Institute of Civil Engineers, and a Fellow of the Geological Society.

Admiral William Henry Smyth was born at Westminster January 21st, 1788. He was the only son of Joseph Brewer Palmer Smyth, Esq., of New Jersey, and Georgina Caroline, granddaughter of the Rev. M. Pilkington. He was a direct descendant of the celebrated Captain John Smith, who has been termed the "Saviour of Virginia." During the American War of Independence Mr. J. B. P. Smyth warmly espoused the cause of the mother country; but the success of the revolutionists was fatal to his fortunes, and the only compensation his family ever received for the loss of large possessions, was the granting of a small annuity to Mrs. Smyth after the death of her husband, which followed closely on the termination of the war. At an unusually early age young Smyth followed the bent of his ardent predilections for a maritime life by shipping on board a West India merchantman, which for-

tunately was commanded by an intelligent Master in the Royal Navy, who took some pains in teaching his *protégé* the rudiments of seamanship and navigation; and it may safely be asserted that he had an apt and ready pupil. He afterwards served in the East India Company's ship Cornwallis; and this vessel being purchased by his Majesty's Government and commissioned as a frigate under the command of Captain (afterwards Admiral) Johnston, his long-cherished wish to serve in the navy was at length gratified. He continued to serve under the same excellent officer in that vessel, and subsequently in the Powerful, of 74 guns, until she was paid off in 1809, during which period he saw much active service in the Eastern Seas, notwithstanding the fact that the ship, during a large portion of the time, was so crazy as to be hardly seaworthy. Mr. Smyth then joined the Milford, of 74 guns, and again became engaged in active service on the French coast; and his vigilance, activity, and courage won the regard and approbation of his superior officers. The Milford was sent to Cadiz in the autumn of 1810, and immediately on arrival, Mr. Smyth was appointed to the command of a large Spanish gunboat (the Mors-aut-Gloria), manned by a British crew of thirty-five men, and mounting a long brass 36-pounder and a 6-inch howitzer In this vessel he took an active part in nearly every operation of the flotilla, operating on the Spanish Coast. The Mors-aut-Gloria was one of a numerous fleet of gun-boats, which, from the dash and energy displayed in handling them, were called "fire-eaters," and among them the Mors-aut-Gloria was especially distinguished. On one occasion, we are told, she fired upwards of seventy rounds, and seemed to attract the particular attention of the French gunners; probably from her superior size, and the conspicuous death's-head and cross-bones with which her bows were decorated, so that their ricochet shot were constantly splashing the spray over her, and cut several of her sweeps; yet, strange to say, she sustained no damage. Notwithstanding the ceaseless vigilance required from Mr. Smyth during these stirring times, he managed to find time for reading and self-improvement; and, indeed, every moment that he could spare from his professional duties was devoted to hard study. Thus early he laid the foundation of his future reputation.

It appears that, on his arrival on the Spanish coast, Mr. Smyth at once embraced every opportunity that presented to make himself acquainted with the hydrography of the district, and the knowledge he thus acquired proved of great service in the prolonged naval operations of the war. Some excellent charts which he constructed of La Isla-de-Leon and the neighbouring coast, showing accurately the strength of the various French and Spanish batteries, having been submitted to Lord Melville, combined with his own distinguished services as an officer, procured him a lieutenant's commission, bearing

date March 15th, 1813, which was accompanied by a note of a very gratifying character from that nobleman.

Almost immediately after the receipt of his commission, he was appointed to a command in the Anglo-Sicilian fleet at Messina under Brigadier Sir Robert Hall; and here we will take the liberty of making an extract from Marshall's *Naval Biography:—*

"One of the first services in which Lieutenant Smyth appears to have been employed was a confidential mission to the Court of Naples, then just wavering in its allegiance to Napoleon Buonaparte. Early in 1814 he proceeded to Palermo in command of the Scylla brig, having Sir Robert Hall's flag on board; and while there, was exposed to a serious personal danger. In the night of the 19th of February, being on shore with the Brigadier, he received a report that the Scylla was in flames. The wind then blew a furious gale, with heavy torrents of rain, and he had the utmost difficulty in getting a boat launched from Porta-Felice. On rowing a little way out, he perceived a large ship in flames and adrift, and that his own vessel was riding in safety."

In April 1814 the abdication of Napoleon put an end for a time to the European war, and Lieutenant Smyth immediately applied himself to a minute survey of the Island of Sicily, of which he executed numerous plans and charts, which were highly approved by Admiral Penrose, who had been appointed to the command of the Mediterranean Station, and were warmly commended by him to the notice of the Board of Admiralty. He also, at his own cost and without official instructions, carried on an extensive series of hydrographical operations connecting Sicily with the adjacent coasts of Italy and with Barbary. These voluntary labours fortunately were appreciated by the authorities at home, who, on September 18th, 1815, advanced him to the rank of Commander, and directed his charts to be engraved in the Admiralty office. It was also arranged that Captain Smyth should publish a full description of Sicily and the neighbouring islands, the Admiralty agreeing to purchase 100 copies. This work was published in 1824 in a quarto volume with numerous illustrations, and from the exhaustive manner in which the subject was treated, and the vast amount of useful information brought together, it attracted much attention both at home and abroad.

He next became engaged, in company with Colonel Hanmer Warrington, in the collection of numerous specimens of ancient architecture from the ruins of Leptis Magna in Barbary, which had been offered to the British King by the Bashaw of Tripoli. These were afterwards deposited in the Court-yard of the British Museum, but were subsequently removed to Windsor, and have served as models from which many architectural decorations have been copied.

From the close of the War to the year 1825 he was almost

uninterruptedly employed in his Surveys of the Mediterranean; a labour, the importance of which has been fully recognised.

It is particularly interesting to Members of the Royal Astronomical Society to observe that it was during the Survey of the Mediterranean that Captain Smyth confirmed that intense love of Astronomy which he retained through life, and to the advancement of which he afterwards devoted many of the best years of his life.

Here is his own account of the method pursued in obtaining the latitudes of the principal points in the Survey:—

"Many of the principal latitudes were taken on shore with the 9-inch quintant and artificial horizon, and with the reflecting circle and sextant; but some of the first-class were obtained with a fine 15-inch altitude-and-azimuth circle, by means of sets, with the face of the instrument alternately turned to the east and the west."

During the prosecution of this survey he formed an intimate acquaintance with the great Italian astronomer Piazzi, for whose talents and character he entertained a high admiration.

In the year 1815 he married Annarella, the only daughter of T. Warington, Esq., of Naples, a lady of great ability and rare accomplishments; and who through all his scientific labours of every description was his devoted companion and assistant.

Released at length by the publication of his *Mediterranean Sea*, he resolved to cultivate the taste for astronomy which he had imbibed, and which, he tells us, "received its sharpest spur at the close of 1813, when I accidentally assisted Piazzi in reading some of the proof-sheets of the Palermo Catalogue." His first intention was to form a standard catalogue of the principal stars in the northern hemisphere, by comparison with the standard Greenwich stars, but fortunately for astronomy he afterwards abandoned the idea of "grinding the meridian." Acquiring a powerful refracting telescope in 1830, he resolved to enter "upon a wider scrutiny of the general sidereal phenomena." Settling at Bedford he matured his plans, and erected the Observatory which has since been so fully described in the publications of the Society, as well as in the work on astronomy which he afterwards published, that a detailed account of it is here unnecessary, but at the time of its erection, and for some years afterwards, it was undoubtedly the most complete and practically useful private Observatory in existence, and has been the model from which numerous other private Observatories have been built.

The principal instrument was an equatorially-mounted refractor of 5·9-inches aperture and 104·5-inches focus, the optical portion of which was considered to be the *chef d'œuvre* of Mr. Tulley; the other instruments were a beautiful 30-inch circle belonging to the Society, and an excellent transit and

clock. It is worthy of remark that the Equatoreal was the first, or at any rate one of the first, in this country to which a clock-work driver was applied, the apparatus being devised by the Rev. Richard Sheepshanks, and by him presented to Captain Smyth. Having completed the erection and furniture of his "Uranian Temple," as he was wont to term it, he devoted himself, with an energy which has rarely been excelled, to the micrometrical measurement of the positions and distances of a numerous list of double and multiple stars, including most of an ascertained or suspected binary character, and also to physical observations of a selected list of clusters and nebulæ, from the catalogues of Messier and the two Herschels. The objects observed were as follows:—

Nebulæ	98	Binary Stars	20
Clusters	72	Triple Stars	46
Stars and *Comites* ..	161	Quadruple Stars ..	13
Double Stars ..	419	Multiple Stars ..	21

The more interesting binary stars were observed in some instances annually, and others were scrutinised at wider intervals. The whole of this work presents an instance of constant and sustained labour for a definite and distinct purpose during twelve years such as has not often been excelled in the annals of extra-meridional astronomy.

The object for which the Bedford Observatory was designed having been completed, it was dismantled in 1839; the Equatoreal was purchased by Dr. Lee, and re-erected at Hartwell House in a remarkably well-appointed Observatory designed by Captain Smyth. In making the transfer to the Doctor, Captain Smyth stipulated for the occasional use of his old favourite, which was readily conceded; and during a period of twenty years he continued to make extensive observations with it, especially after taking up his residence at St. John's Lodge, within a short distance of Hartwell.

In 1839 Captain Smyth removed to Cardiff, in order to superintend the construction of the immense floating-docks at that port by the Marquis of Bute, but he employed all his leisure hours in arranging and reducing his Bedford observations for the press. These were all embodied in the work now so familiar to the amateur astronomer, which appeared in 1844, entitled *A Cycle of Celestial Objects*, a work justly esteemed as one of the most captivating and popular treatises on elementary and especially sidereal astronomy in our language. The second volume of this work forms the "Bedford Catalogue," of 850 celestial objects arranged in order of right ascension, and embodying the results of all his micrometrical and other observations. The descriptions of the various objects are enlivened with a vast amount of general classic and antiquarian lore, introduced in a most genial spirit. The astronomical value

of the Catalogue is of course mainly dependent on the accuracy of the micrometrical measures. This has been severely tested by many observers of experience; and it may safely be said that in this all-important element the *Cycle* will stand the most critical tests that can be applied. On the appearance of the *Cycle of Celestial Objects* the Society marked their appreciation of its value by awarding their gold medal to its author. In presenting the medal Mr. Airy, the President for the year, used these emphatic words:—

"My confidence in the exactness of the observations is purely personal. Knowing the attention which has been given to the instrumental adjustments, the intentness of the observer upon his work, the nerve, which is made steady rather than disturbed by the anxiety to procure a good observation, and the general skill in the management of the instruments, I can truly say that if an accurate observation were required I should desire that it should be made by Captain Smyth." In handing over the medal he added, "And I beg leave to convey with it the expression of my own opinion that never was a medal more worthily earned." It is twenty years and more since these words were uttered, but every observer of double stars, every amateur astronomer, will heartily endorse these sentiments.

The publication of the *Cycle* by no means terminated the astronomical labours of Captain Smyth. For many years, when leisure permitted, he returned to his telescope with all his old zeal and accuracy; and his observations, entirely on objects in the "Bedford Catalogue," were afterwards partly published in a quarto volume, entitled *Ædes Hartwellianæ*, but afterwards in a more complete form in a work entitled *Speculum Hartwellianum*, which appeared in 1860, gracefully dedicated to those who had in any way assisted him in his researches. This work is a supplement to the "Bedford Catalogue," containing the results of observations made at Hartwell between the years 1839 and 1859, bringing up to the latter date the history of many of the more remarkable double stars. Among them γ *Virginis* occupies a conspicuous place. Captain Smyth fortunately observed the perihelion passage of the comparison star in 1836, and with such rapidity was it performed that, although its period is probably somewhere about 180 years, he has actually measured it through a course of position angle of 270°! From Captain Smyth's measures alone (but we must now term him Admiral, for he was gradually promoted to the highest rank) Mr. Hind computed the elements of its orbit. The result was in remarkable accordance with the elements deduced by our ablest astronomers from a consideration of ALL the recorded measures of γ *Virginis*. This must be accepted as decisive testimony to the accuracy of Admiral Smyth's measures, as well as to his sustained vigilance and skill through a period of thirty years.

The same volume contains a history of all the circumstances attendant on the discovery of the planet *Neptune*, and from the fact of his being President of the Society during the warm and animated discussions which this discovery gave rise to, he had ample opportunities of hearing and weighing every point in the controversy. For close logic and terse statement of fact the article in question can hardly be excelled in the circle of astronomical literature, and it is impossible to rise from its perusal without a conviction that it is the production of a fearless and able man, anxious only for the truth.

The Admiral's last astronomical work was a little *brochure* entitled *Sidereal Chromatics*, which also may be considered as supplementary to the *Cycle*. It contains many curious facts and interesting speculations on the subject of the colours of stars, and a first attempt to verify the tints by reference to a distinct and well-defined chromatic scale.

Such was Admiral Smyth as an observer and astronomical writer. It is not our purpose to trace his career except as an astronomer, and, as a member of our Society, further than is necessary to a correct appreciation of the MAN. He has left his mark on all he touched. As a geographer, a hydrographer, a numismatologist, and an antiquarian, he was equally distinguished by the depth of his inquiry, his untiring industry, and the sagacity of his deductions. A reference to his works on the Mediterranean and on Ancient Medals, will at once satisfy the inquiries on these points.

As a member of the Royal Astronomical Society his memory will ever be revered. He joined the Society in 1821, very shortly after its first establishment, and from that time to a very recent period he served it either in the capacity of Member of the Council, Foreign Secretary, Vice-President, or President. How he discharged the various duties which these several positions placed him in can now be only known fully to some of the older members of the Society. For many years he attended constantly both our Council meetings and our ordinary meetings. As President during the exciting period before alluded to, he displayed to advantage the many admirable qualities of his mind, and by his obvious fairness and impartiality, though holding strong opinions himself, he guided the Society in safety, and left on the minds of those engaged in the discussions of the period the conviction that every body and every opinion had had fair play.

He possessed most of the qualities necessary for President of a body like our Society, ample information on every subject that arose, a memory rarely at fault, courage of the highest kind, and that happy blending of firmness and good-humour which both commands and wins. It may be said, with perfect truth, that the Society has never numbered among its members one more anxious for its interests, more faithful in services, or more jealous of its honour.

After "the Admiral" (as he has been almost invariably termed of late years) removed to St. John's Lodge, the distance from town and a tendency to bronchitis rendered his attendance at our meetings less constant than heretofore, but his interest in our welfare was unabated, and his counsel often sought and followed.

For many years he was a member of the Board of Visitors of the Royal Observatory, where he was always attentive to business, and gave his hearty support to every measure which tended to promote the efficiency and usefulness of that establishment.

As President of the Astronomical Club, he was always genial and courteous, ever keeping things in happy order, and by his ready wit and flow of humour compelling the maintenance of good fellowship.

During his residence in London, he was a leading member of various other scientific societies, and in every one he connected himself with he became at once an influential member. Of these we may enumerate the United Service Institution, of which he may be said to have been the founder. He was a fellow of the Royal, the Geographical, and Antiquarian Societies. Of late years, though with mental vigour unimpaired, and a capacity for work as strong as ever, he led a life of comparative retirement, yet preparing a MS. which is now in type. He felt the necessity of quiet after so prolonged a life of activity, and this feeling was deepened by the sudden loss of a beloved and accomplished daughter a few years ago,—a loss from which it may be doubted if he ever fully recovered.

He still occasionally came to town, but, as he expressed it, he "felt his days were over" for taking a prominent part in public affairs. During the last few years of his life he suffered at times from a painful disorder, but his general health was good, and his mind as strong as ever. In the spring of last year it became evident to his family that age was beginning to tell upon him to some extent, but it was not until within the last two or three weeks of his life that any fears of an impending change were entertained. Early in September he had an attack, from which, however, he seemed to recover in a great measure, and was able to drive out, and to move about in the house pretty much as usual. On Friday the 8th of September, he was able to take a short drive; in the evening he was cheerful as usual, and sufficiently well to be able to adjust a small telescope, and show the planet *Jupiter* to his little grandson, Arthur Smyth Flower, with whom he chatted in his usual playful manner when talking to children. This was almost the last act of his life. He retired to bed at his usual hour, and was able to undress without much assistance. A few hours afterwards he was seized with a sudden suffusion of blood on the lungs, and shortly afterwards, peacefully and without a struggle, his noble and

gentle spirit passed to its eternal rest. It was a death such as he wished to die. He dreaded a lingering illness with gradually decaying mental and bodily faculties. Conversing with the writer of this memoir on this subject some years ago, he expressed himself with much feeling, and in his own expressive phraseology added, "I trust when the 'Fell Sergeant' does come to me he will strike home." His death occurred early in the morning of September the 9th, 1865, and a few days afterwards his remains were laid by the side of his beloved daughter in the little churchyard at Stone, near Aylesbury. He was in the 78th year of his age.

It may truly be said of him that whatever he did he did it with his might. When at his work he tolerated no interruption, but when his day's work was done he was the most joyous of companions. He had a great fondness for children, and used to fill his pockets with new half-pennies, to distribute to those he met in his daily walks. To the young astronomer he was ever ready to lend a helping hand, and when a new observatory was to be built, he was generally consulted.

His letters were very characteristic of the man,—short, sharp, always to the point, and sailor-like in phrase. His handwriting was as clear and legible as the best print, every letter was distinct, and if ever handwriting showed the character of the writer it was his.

Admiral Smyth has gone from among us, but he has left behind him a memory which will long be cherished, and an example which cannot be too closely followed. Every member of the Society will join with your Council in the expression of sympathy with his bereaved widow and family, and in their admiration of one who was both great and good.* [I. F.]

John Francis Encke, born Sept. 23, 1791, was the youngest son but one of the deacon of the Jacobi Church in Hamburg. Four years after his birth his father died, leaving the care and the education of eight children to his mother, a lady of much worth, and happily possessed of great mental energy.

The first tutor of the boy was Mr. Hipp, a gentleman possessing considerable aptitude for mathematical teaching; and to his honour be it spoken, a man who rendered valuable pecuniary assistance to the orphan and moneyless family. Hipp continued this material encouragement to young Encke even after the time that he entered the College at Hamburg, well known as the Johanneum. At this College, then under the directorship of Gurlitt, who enjoyed a high reputation for classical learning, the boy-student rapidly advanced, and in

* The writer acknowledges his obligations to Sir John Herschel and the Astronomer Royal.

addition to considerable ability in Latin composition, his knowledge of Greek was sufficient to enable him to translate and enjoy the Lyrics of Pindar. Notwithstanding, however, this early classical training, when the time came for his entrance at the University, Encke resolved henceforth to devote his attention mainly, if not exclusively, to the study of astronomy.

But here came a very formidable impediment; there were ample funds at the disposal of a poor clergyman's son for a theological career, but none for the prosecution of so unusual a study. Nevertheless, such was the acknowledged ability, and so determined was the inclination, of young Encke, that, as is happily not unusual in such cases, all the difficulties yielded at length to perseverance, and to his great joy, in Oct. 1811, he found himself at Göttingen, and a student under the celebrated Gauss.

The very newspapers of Hamburg were at that day compulsorily printed in French; as a condescension, however, or as an insult to the inhabitants, a German translation was added; in a like spirit even the university matricula of the old "Georgia Augusta" of Göttingen had the image and superscription of Jerome Buonaparte printed upon it. No wonder then that neither Gauss nor astronomy could retain the young student at his books, but, obeying the impulse which animated the whole heart of Germany, in the spring of 1813 he took up arms and marched to Hamburg for the rescue of his country from the domination of the French. After the re-occupation of Hamburg by the foreigner, Encke entered the Hanseatic Legion, then in process of formation in Holstein and Mecklenburg, and there he served as a sergeant-major in the horse artillery until July 1814. In the autumn of this year he returned to Göttingen and to his astronomical pursuits, and for nearly twelve months continued a diligent student of subjects far more peaceable, and far more congenial to his turn of mind. Nevertheless the return of Napoleon from Elba once more finds him in a soldier's uniform, but now only for a short period, and, happily, for the last time. Waterloo and its consequences restored peace to France and to Europe, and young Encke, who in peace had no taste for soldiership and a uniform, returned, for the third time, to Göttingen and to Gauss. It was thus in the midst of these stirring and troublesome events, that the spirits of such men as Franz Encke and Wilhelm Struve were disciplined and matured.

While Encke was serving as a lieutenant of artillery in the Prussian fortress of Kolberg, he became acquainted with the celebrated Lindenau, at once astronomer and statesman, and after the completion of his studies under Gauss, he was appointed, by the influence of the former, an assistant in the observatory of Seeberg, not far from Gotha. In 1820 he became Vice-director, and in 1822 he was appointed Director, in the place of Lindenau, who returned to his political career.

It was at Seeberg that Encke commenced and completed his important work on the "Transits of *Venus* in 1761 and 1769," published at Gotha in 1822 and 1824. He also matured his investigation of the Comet of 1680, and of the remarkable comet of short period which bears his name. Zach's *Correspondence* and Lindenau's *Zeitschrift*, about this period, contain many evidences of his talents and his industry. During his directorship of the Observatory at Seeberg he was elected an Honorary Associate of the Royal Astronomical Society, and at the time of his decease was the oldest foreign member on our list. In 1824 the Council of our Society awarded to Encke their gold medal for what Mr. Colebrooke, the President of that day, properly designated as "the greatest step that had been made in the astronomy of comets since the verification of Halley's Comet in 1759." Encke had long been on the track of his comet. In 1818 he had succeeded in identifying it with the Comet of Mechain and Messier in 1786, and again with the comet discovered by Miss Herschel in 1795, and with the Comet of Pons in 1805. The result of his investigations was, that this comet, which astronomers have agreed to designate as "Encke's Comet" (although he himself always modestly calls it the Comet of Pons), would make its appearance again in 1822, although it would not then be visible in Europe. Accordingly our Society had the gratification of presenting to Mr. Rümker their medal for its discovery at Paramatta in 1822, on the same day when they bestowed a similar mark of approbation, as we have already stated, on Encke himself, for its prediction.

It was in these Memoirs, that Encke signalised himself by his systematic and most successful application of the *principle of least squares* to a number of astronomical observations. For the method itself we are mainly indebted to Legendre and to Gauss, but for the first exhibition of its vast practical value, we are indebted to the example of Encke. His mind, indeed, seems to have been pre-eminently arithmetical, delighting in the orderly and systematic development of what otherwise and to many would seem an inextricable maze of figures. Those who knew him best consider that he probably injured the generality of his mathematical analysis by the fastidious care which he bestowed upon its symmetrical arrangement.

In 1825, at the recommendation of Bessel, Encke was appointed to the Directorship of the Observatory at Berlin; the observatory itself was both improperly situated, and inadequately supplied with instruments, but ultimately, at the suggestion of Humboldt, a new observatory was erected at the expense of the Prussian Government, Encke superintending personally both its construction and its interior arrangements. And here, for eight or ten years after its completion, he continued with much assiduity to observe both with the Transit Circle and

the Equatoreal; but his natural tastes did not lie in instrumental observations, and after the discovery of numerous small planets by various observers, he devoted himself with much success to the investigation of planetary disturbances.

The labours of Encke in reference to the Comet which bears his name have already been referred to. Having carefully taken into account the perturbing action of the planets on this comet during several successive periods, he established the remarkable fact that there is some extraneous cause in operation which continually diminishes the comet's periodic time. This is evidently the effect which would be produced if the comet suffered a resistance from moving in a very rare ethereal medium, and accordingly this is the explanation proposed by Encke, and at present generally accepted by astronomers.

Encke has also, as already mentioned, devoted special attention to the subject of the perturbations of the Minor Planets.

In the Appendix to the *Berliner Jahrbuch* for 1837 and 1838, he expounds in detail the method of calculating these perturbations which had been long used by himself and other German astronomers, and which was originally given by Gauss. In this method the perturbations of the six elements of the orbit are computed for successive equal intervals of time by means of mechanical quadratures, and from the values of the elements thus found for any given time, the co-ordinates of the body at that time are determined.

Now this method, although a very beautiful one in theory, is attended with the disadvantage of requiring the determination of double the number of unknown quantities that are really wanted, and the calculations which must be gone through consequently become excessively long.

As the number of the known minor planets become larger, the want of a readier method of computing their perturbations became more and more pressing.

Encke was thus impelled to devise a mode of applying the method of integration by quadratures directly to the differential equations of motion of the disturbed body, and he published an account of this new method in the Proceedings of the Berlin Academy for 1851. In this Memoir he refers the place of the body to rectangular co-ordinates, and he determines the perturbations of its movements during successive short intervals of time by a direct computation of the changes produced in the three co-ordinates by the action of the disturbing planet.

He estimates that the labour of computation is reduced by the new method to less than one-half of that required by the method previously employed.

It should be remarked that Prof. G. P. Bond, in a paper which was communicated to the American Academy of Arts and Sciences in 1849, had already briefly explained a method of calculating perturbations exactly similar in principle to that

of Prof. Encke, but the latter was totally unaware of the existence of this paper when he published his own Memoir, which enters much more fully into the practical details of the method, and gives greater prominence to the importance of it as applied to the case of the minor planets.

By astronomers of the present day it is possible that Encke may be most highly estimated for the vast improvements which he introduced into the Berlin Ephemeris. The history of astronomical ephemerides is not a little varied and curious; a concise account of it will be found in the fourth volume of the *Memoirs* of the Royal Astronomical Society, on the occasion of the Council of the Society presenting Encke, through their President, with a gold medal, for the part which he had taken in the improvement of the Berlin Ephemeris. Our own *Nautical Almanac*, at that day, viz. in 1830, had fallen or had remained greatly behind the requirements of Astronomers; but in speaking of the merits of the foreign Ephemeris, the report of the Council runs as follows: "A gold medal has been voted to Professor Encke for the superb *Ephemeris of Berlin*. It would be superfluous to dwell upon the merits of this well-known work, which, far outstripping all rivalry, must be considered as the only Ephemeris on a level with the present wants of the sciences." On presenting the medal, Sir James South, the President, adds, "With the *Berlin Ephemeris*, an observatory scarcely wants a single book; without it, every one." It would, however, be disloyal, though in any other aspect it may be needless, not to add that what has just been said of the Berlin Ephemeris of 1830, may with equal truth be predicated of the *Nautical Almanacs* from 1834 to the present date; nevertheless the first impulse came from Encke and Berlin.

Many other labours of Encke may also be found in the Memoirs and Monthly Reports of the Berlin Academy, in the *Astronomische Nachrichten*, and in four volumes of the Berlin Observations. He is also well known by the publication of several excellent speeches, and especially for a memorable *éloge* on the celebrated Bessel.

Encke visited England in the autumn of 1840, in order to be present at the meeting of the British Association, and for the purpose of inspecting the English Observatories. His account of that journey is a testimony of the deep and pleasing impression which his hearty reception in England left upon his memory.

In 1859 Encke suffered from an apoplectic fit, and foreseeing the commencement of disease of the brain, he obtained leave of absence from his observatory in the spring of 1863. In the autumn of the same year, finding a recurrence of the same symptoms, and knowing what they implied, with a brave heart, the now aged man explained his forebodings to a physician, and at once placed himself under his care in an institution for

diseases of the brain at Kiel. At the commencement of 1864 he requested permission to be relieved from all astronomical work, and until the time of his decease, continued to live in a quiet, happy state of mind, in the midst of his family, at Spandau, near Berlin.

Encke, during the forty years of his professorship at Berlin, impressed the form and bent of his mind upon many pupils, who have ably contributed their share in the progress of astronomical knowledge. There is no greater proof of the real worth of a teacher, than when his pupils speak well and lovingly of him. They see the man in his weakness and in his strength. So it fared with Encke. They bear strong and uniform testimony to his eminent frankness and truthfulness; his labours, they say, were incessant, his recreations few; he was simple in his manners, and in all his habits temperate. Towards his coadjutors and assistants he showed a severe judgment, but he set them a severer example. A man such as this, absorbed in his work, and shutting himself away from the outer world, was likely to be sometimes abrupt, or laconic, or even incautious, in his utterances; these utterances, from their bluntness or their truthfulness, occasionally gave offence, and involved Encke in trouble. As age, however, grew upon him he became more gentle in his manners, and softer in his address; and in the presence of those whom he knew and trusted, the old man would sometimes review his own life, and urge his favourite pupils to draw from his own experience lessons of moderation and self-restraint, both in passing their judgments on the labours of others, and in the amount of labour which they felt it their duty to exact from themselves.

There occurs but one more question regarding this great and venerable man; the writer of this memoir gladly adopts this language, *great and venerable*, because they are the very words selected by men who served him long and who knew him well, and who are themselves doing good public service in their own day. It is well known that great theological activity, not to say theological strife, surrounded Encke and every other intellectual thinker in Germany; it may not, perhaps, concern us, simply as students in Astronomy, but it cannot fail to interest us as men, to know what effect this independence of thought and boldness of expression had upon the spirit of a man, whose name will for ever be associated with some of the noblest and furthest-reaching efforts of the human mind. In reply to this question, we are told by those who knew him intimately, that Encke retained through life the strength and simplicity of his early faith; and we also learn that he was heard repeatedly to say, that one of the greatest pleasures of his life was derived from the fact, that one of his sons had become a minister of the Gospel. [C. P.]

Proceedings of various Observatories.

Royal Observatory, Greenwich.

The work of the Royal Observatory, Greenwich, during the past year has been of the usual character. The Moon has been observed on the meridian, with the transit-circle, and off the meridian with the altazimuth, at all possible opportunities. The principal planets have been observed on the meridian with the greatest regularity. In accordance with a convention between the Astronomer Royal and the Director of the Imperial Observatory, Paris, the asteroids have only been observed at Greenwich during the first half of each lunation. They have been observed at Paris during the second half of each lunation.

Considerable progress has been made in obtaining observations of the stars contained in Bessel's *Fundamenta*, which had not been previously re-observed at Greenwich since Bradley's time.

Many observations of stars have been obtained at the request of Astronomers who have required places fixed by the Greenwich Transit for their particular investigations.

The new value of the Mean Horizontal Solar Parallax 8″·94 has been adopted for use in the Observatory. It has also been adopted for use in the Imperial Observatory, Paris.

The galvanic connexions have remained in perfect order and without material extensions or changes during the past year.

The photographic self-registering apparatus for the indication of Earth currents has been brought into use, and has furnished some most interesting records during the past year.

The work of the Observatory will be seen to have been chiefly directed to the storing up materials for future use in those branches of Astronomy which fall least readily within the reach of the Amateur.

Radcliffe Observatory, Oxford.

In this Observatory no changes of an organic character have taken place either in the system or subjects of observation, or the personal staff.

The year, as regards weather fit for observing purposes, has been below the average, especially during the early and late portions of it, and this has affected the number of observations, but not to a very great extent. With the Carrington Transit Circle, there have been observed 1043 objects, including 113 observations of the Sun, 55 of the Moon, 25 of *Mercury*, and 18 of *Mars*. The observations of *Venus*, *Jupiter*, and *Saturn*, have been given up, as there seems no particular use in mul-

tiplying observations of these bodies, those made at Greenwich being amply sufficient for all purposes to which they may be applied. It seems, however, to be desirable that observations of the position of the Sun should be generally made for the purpose of securing an independent determination of the equinoctial points of the ecliptic ; and, with regard to the Moon, it may frequently happen that an observation may be made at an Observatory on a day when clouds have prevented it at another. The same remark applies to observations of *Mercury*, which are necessarily scanty, and in which the Radcliffe Observatory has been particularly successful.

The heliometer has been used by Mr. Main for observations of Struve's *Lucidæ*, as in the preceding year; but, nearly all the stars in Struve's first Appendix, at large distances from each other, have been added to the list, together with the stars having conspicuous proper motion included in his second Appendix.

Some occultations of stars by the Moon have also been observed with the heliometer telescope, and thoroughly reduced. The heliometer is now in a very complete state of repair and adjustment. The error of elevation of the areas which existed in former years has been accurately corrected, and a much simpler means of conveying the galvanic current for the illumination of the scale has been adopted. Failures of the light rarely occur now, and when they do happen, they are generally traceable to the battery, to which a very simple and easy test is applied.

The meteorological observations and discussions have been kept up with the same vigour and success as in former years ; and in the records of recent gales, which have been unusually protracted and severe, a good stock of materials exists for comparison with other Observatories, with a view to the advancement of the theory of storms.

The reductions of the astronomical observations are in a very advanced and satisfactory state, Mr. Main having devoted a great portion of his own time to them.

The reductions of the astronomical observations of 1864 are completed, as well as the Right Ascensions of 1865, excepting in the latter case the completion of the reductions to mean right ascension of the stars not used for clock or azimuthal error. The Right Ascensions of 1864 have also been written out into ledgers, and compared with the B. A. C. and other Catalogues; and the catalogue of all stars observed in 1864 (including 1208 stars) has been formed.

All the observed Right Ascensions of planets to the end of 1865, have been compared with the *Nautical Almanac*, and the mean times have been computed.

The printing of the astronomical portion of the volume of the *Radcliffe Observations* for 1863 is very nearly completed ; and Mr. Main was enabled, some time since, to distribute a

few copies of the Catalogue of 1115 stars observed in that year.

A little has been done additionally towards the completion of the Catalogue of Stars observed between the years 1854 and 1861, but the necessity of providing for the daily routine of work, and the reduction of the observations, taxes the energies of the small personal establishment of the Observatory to such an extent, as to leave little time for other subjects which admit of some delay.

Cambridge Observatory.

The objects kept mainly in view in the Cambridge Observatory for the year 1865 are —

1. A determination with the meridian instruments of the stars to which M. Oeltzen has assigned considerable proper motions, by a comparison of the *Histoire Céleste* with Argelander's Northern Zones. The original intention of obtaining five independent observations of each star with the transit instrument and mural circle has been adhered to, and a large proportion of the requisite observations is now completed.

2. The observations of Argelander's list for standard stars, given in the *Ast. Nach.* No. 1540.

3. The usual observations of the Sun about the times of the equinoxes and solstices, and of the larger planets at opposition.

4. The Northumberland Equatoreal has been employed chiefly in observing Faye's Comet, seeking for Biela's, observing the occultations of stars by the Moon, and examining the surface of the Sun. Suitable improved eye-pieces have been provided and used for these purposes.

5. The reductions of the observations, which had fallen considerably into arrear

The means of the transit and circle observations are taken, and the corrections for irregularity of pivots in the former and for runs in the latter are applied to the latest date of the observations. All the small corrections for the Transit Instrument are applied; the clock errors, obtained to the end of August 1865, and the true north polar distances to the end of 1864.

Some changes have been made in the observations, furnishing data for instrumental corrections. The pivots of the Transit Instrument have been carefully examined through the entire revolution, at equal intervals and at important points, and some interesting results are the consequence. The collimation errors are obtained independently of reversal, very frequently in each position of the instrument, by the aid of Bohnenberger's eyepiece, and by direct and reflection observations of slow stars, chiefly *Polaris* and δ *Ursæ Minoris*. In fact, this and all the

other small corrections are found out, as far as possible, every night that is favourable. As there are frequent and irregular changes in the runs of the microscopes of the Mural Circle, it has been the practice for some time to take the runs with each observation.

Royal Observatory, Edinburgh.

Mr. Piazzi Smyth states that the only instrumental changes in the Royal Observatory, Edinburgh, since the last Report, have been two clock-improvements of local invention. The first is a dynamical regulation of ingenious construction by Messrs. Ritchie and Son, to the escapement of the "loud sidereal seconds-striking clock," for the equalization of its adjacent beats; which are now brought by this contrivance as close to perfection in that way, as the transit-observer can possibly desire. The next is a new, and in practice most satisfactory, method by Mr. Lang, both for first bringing a normal mean-time clock to a small rate without stopping it to make alterations on the pendulum, and then correcting its indications each day, if required, to any small fraction of a second without interfering with the goodness of its rate during the remaining twenty-four hours.

Observations of star-places with transit instrument and mural circle have been going on as usual; together with the furnishing of the public with true time through means of the three several methods of time-ball, time-gun, and controlled-clocks.

The reduction of meteorological observations for the Registrar General of Scotland has also been in continual progress, and occupies a large share of time, the number of observing stations being fifty-five; and it is only too apparent that until meteorological theory is very much improved, the number cannot be reduced.

Thanks to the excellent performances of their duties by the two assistant astronomers, the above never-ending tasks were not sensibly interfered with by the absence of the Astronomer Royal of Scotland in Egypt during several months of last winter and spring. This visit had resulted from an inquiry which circumstances had called on him to take up connected with the principal observational facts, many of them astronomical, of the Great Pyramid; an inquiry merely literary at first, but soon demanding an actual repetition of the chief observations at the place; so that it was rather fortunate it should have fallen on the only Director of an Observatory who is officially non-resident.

The observations,—which were much facilitated by the liberal condescensions of His Highness the Viceroy of Egypt, and which acknowledged the importance of the three great epochs of modern investigation there, viz., Professor Greaves in 1639,

the French *savans* in 1799, and Colonel Howard-Vyse in 1837, —were mainly directed in three departments of linear, angular, and thermal measures, with special reference to all cases of discordance amongst former observers, as well as the recovery, where possible, of ancient fiducial marks or surfaces ; in connexion with which it may be mentioned, that the astronomical bearings of the corner sockets cut in the natural rock of the hill, marking the original size of the finished monument, were determined on two sides of the Pyramid by means of a powerful altitude-azimuth circle reading by microscope micrometers. This instrument was likewise carried to the top of the Pyramid, and also into the interior, and wherever there were important angles requiring accurate measurement.

The investigation lasted through 110 days of continued work on the spot, and its records are now in course of preparing for publication.

Glasgow Observatory.

The operations at the Glasgow Observatory during the past year have been mainly of the same character as in former years. The transit circle is still employed in the observation of stars included chiefly between the sixth and ninth magnitudes. A few observations of the minor planets have also been made with the instrument. The Ochtertyre Equatoreal has been used to a great extent in micrometric measurements of double stars, a list of such objects having been selected for the purpose from Struve's great Catalogue. The chief novelty of the operations during the past year has been the determination of the longitude of the Observatory, by means of galvanic signals exchanged on several nights with the Royal Observatory, Greenwich. An account of this operation, the result of which was very satisfactory, has been recently published in the *Monthly Notices*. The arrangements for the transmission of true time to the city and port of Glasgow which have occupied so much of Professor Grant's attention during the last two years are now established on a permanent footing. Jones's invention for regulating clocks by electricity is used on an extensive scale in connexion with the Observatory, and the results afford a complete confirmation of Mr. Hartnup's previous experience, as regards the admirable precision and practical utility of the method.

Liverpool Observatory.

At Liverpool, Mr. Hartnup has been engaged during the past year in making arrangements previous to the removal of the Observatory from the Waterloo Dock pier-head to Bidston, about three miles west of its present position. Considerable delay has been caused by the excavation of stone for the new

building, from the site on which it is now being erected. Some practical good will result from this having been done, as the underground accommodation which has been provided will be of great advantage for all investigations requiring uniformity of temperature. The space around the building is well adapted for placing the various meteorological instruments in such positions as to insure results uninfluenced by surrounding objects.

Preparations have been made for determining the difference of longitude between the old and new stations, and observations will be commenced as soon as the new transit-pier is covered in, so as to protect an instrument which will be temporarily mounted there. The results of thermometrical observations taken at both stations during the last twelve months show that the temperature of the air is lower, and the daily range much larger, at Bidston, than it is on the Waterloo Dock pier-head.

The establishment of a depot near the margin of the river for the reception and safe custody of chronometers, and for supplying such rates as can be obtained during the uncertain time that ships remain in port, has not yet been decided on. The necessity for such a provision would be greatly diminished if all chronometers were tested periodically, and mariners supplied with tables of existing errors.

The Greenwich mean time can be obtained with so much facility from the time-balls and clocks in Liverpool, that there is no difficulty in finding the rate in the temperature that prevails at the time; but without a knowledge of the corrections due to change of temperature, it is impossible, when the rate is found in a low or a medium temperature, to say what the alteration will be when the timekeeper is exposed to the heat of a tropical climate.

Kew Observatory.

Mr. Balfour Stewart, Director of the Kew Observatory, reports:—

The Kew Photo-Heliograph has been uninterruptedly at work under the direction of Mr. De La Rue since the last report, and during the year 1865 as many as 272 pictures of the Sun have been taken. The numbers of groups observed in Kew and in Dessau will in future be annually published in the *Monthly Notices* of the Royal Astronomical Society in the same manner as Hofrath Schwabe has done for the past forty years in the *Astronomische Nachrichten* with such benefit to science.

The work of measuring and reducing the Sun observations, as carried on in Kew, has principally two objects in view; first, to bring out in a proper way a great number of facts calculated to throw light on Solar Physics, and, secondly, to collect as many accurate observations as possible, in order to

improve the assumed elements of rotation of the Sun, a problem which the recent and beautiful researches of Carrington have rendered more important than ever. A first instalment of results bearing on the former part of the work has been published very recently under the title "Researches on Solar Physics by Warren De La Rue, Balfour Stewart, and Benjamin Loewy, Series I." The results of this investigation appear to confirm the hypothesis of Wilson, in which spots are regarded as hollows in the luminous surface of the Sun; and they also appear to render it probable that the various degrees of luminosity observed on the Sun's disk are all due to one cause, namely, the presence to a greater or less extent of a comparatively cold, absorbing atmosphere. The second series, which is based on measurements of the areas covered by the nuclear and penumbral parts of all spots, is in active progress. This part of the work embraces the whole of Mr. Carrington's and of the Kew pictures, and will not only be the groundwork of the immediate investigation for which it is intended, but promises to extend the foundation for those inquiries which connect magnetical with solar phenomena.

The Kew researches have greatly gained in scope by the munificent present of all his observations by Hofrath Schwabe, who desired to make them available for the Kew observers, and also by the liberality with which Messrs. Carrington and Howlett have lent their excellent records, thereby placing a most unique collection of material at the disposal of the observers. The access granted to the authors by the Royal Astronomical Society to the observations of Pastorff and Shea will also doubtlessly prove of great advantage.

For the purely astronomical part of the work which the Kew Observatory has undertaken, a great many pictures have already been measured and reduced; and since great care is taken to perfect the method of observation and render the photographic record as good a basis for calculation as the most delicate astronomical observation, there is every reason to hope that this part of our knowledge will also derive benefit from the labours of the Kew observers.

A new collimator arrangement for testing sextants, devised by Mr. Cooke, is in course of construction under the superintendence of Mr. Francis Salter, and will shortly be erected at the Observatory, so that nautical men will then be able to have their sextants tested in an accurate manner. It is also the intention of the Kew Committee to offer to travellers and scientific observers an opportunity to make themselves acquainted at the Observatory with portable astronomical instruments of every kind, with the mode of using them properly, and the best methods of reducing observations. It is unnecessary to dwell on the great benefit which it would confer on astronomical science, if good use could be made of such an opportunity.

The fundamental determinations for the series of pendulum observations to be made in India in connexion with the great Trigonometrical Survey, and for which the Kew Observatory will form the base-station, have been completed, and a report of the experiments, submitted to the Royal Society by Messrs. Balfour Stewart and Loewy, has been published. The instruments have arrived in the best state in India, and the communications which these gentlemen have received from Colonel Walker and Captain Basevi show that the work there has already begun and is actively carried on.

Mr. De La Rue's Observatory.

At Mr. De La Rue's Observatory at Cranford the ordinary routine work has been carried on, and photographs of the Moon in certain phases have been made from time to time, in order to fill up lacunæ existing in the extensive series already procured. On October 4th photographs were obtained both before and during the Eclipse of the Moon, which was nearly in a state of mean libration; the pictures procured before the eclipse are now at the disposal of Mr. Birt, for the preparation of the skeleton chart of the British Association. The pictures obtained during the eclipse were found to be in stereoscopic relation with those of February 1858, so that when they are combined with each other, stereoscopic pictures of phases of a lunar eclipse are presented.

A diligent but ineffectual search was made by Mr. De La Rue and his assistant, Mr. Reynolds, for Biela's Comet on several occasions, on some of which the nights were remarkably fine. The sweeps were so extensive and so carefully gone over, that if the comet had been visible with a 13-inch reflector, it would have been discovered, even if considerable errors had existed in the ephemerides.

An Observatory has been built in a position $3''{\cdot}6$ to the north and $0^s{\cdot}23$ to the west of the old Observatory, in which has been erected the Equatoreal formerly belonging to the late Mr. Palmer. The object-glass of this telescope, by Merz, proves to be a very fine one, but the stand is far too weak, and will have to be replaced by a better mounting. The entire Observatory, walls and dome, 12 feet in diameter, revolves easily on a rail fixed to the floor: it is octagonal in form; two broad ribs extend from one octagonal face to that opposite to it, and carry the rails on which the curved shutter travels; this shutter, 2 feet 6 inches in the clear, is moved by a drum, and a handle carrying a pinion which gears into the wheel of the drum.

Some years ago Mr. De La Rue made photographs of the Sun on a large scale (three feet in diameter) by means of his

reflector and a secondary magnifier; the working of this apparatus presented some difficulties, and he has now having constructed by Messrs. Cooke and Sons, of York, a refractor for that purpose, 13 inches in diameter and 10 feet focal length. This instrument will be specially corrected for the photographic rays, and will serve for taking lunar photographs in the principal focus, as well as Sun pictures, after the rays have passed through the secondary magnifier. It is anticipated that the Sun pictures procured with the new instrument will set at rest many disputed points in Solar Physics.

Lord Wrottesley's Observatory.

At Wrottesley the past year has been mainly devoted to the reobserving such of the double stars contained in Lord Wrottesley's Catalogue of 398, as have rapid orbital motion, or possess other features of interest. The new driving-clock constructed on the model of that of Mr. Dawes, which has been applied to the Equatoreal, is very successful, and the Equatoreal itself has been much improved by the alteration referred to in our *Memoirs*, vol. xxix., p. 86. Although Meteorology does not fall within the province of our Society, it is not out of place to mention that a most valuable communication was made by Mr. Follet Osler to the British Association at their Meeting at Birmingham, describing the results of a careful comparison and discussion of the Anemometer records of Wrottesley and Liverpool. Those persons specially interested in this subject will have an opportunity of reading the paper, which has been ordered to be printed *in extenso* in the Reports of the British Association.

Mr. Fletcher's Observatory at Tarnbank.

This Observatory is probably entitled to rank among the best and most complete observatories in private hands in existence. The chief instrument is a very fine 9·5-inch refractor of 12-feet focus, mounted on a long polar axis, which is probably unique,—it is a single block of cast-iron. The mounting is very fully described in the *Notices* for last June, and need not be further alluded to, except to record that, in firmness and ease of motion, it is completely successful. It is covered by an elliptic dome 18 feet diameter. The other instruments are a 30-inch transit by Simms, and an excellent clock by Frodsham. We are informed that Mr. Fletcher is engaged in re-observing the *Bedford Catalogue*, a work which will occupy some considerable time; and when that is done, it is his intention to bring out a new edition of Admiral Smyth's

Cycle of Celestial Objects, for which purpose the Admiral, some time ago, made over to him his entire interest in that work.

Mr. Huggins's Observatory.

Mr. Huggins has continued during the past year the prismatic researches, some of the results of which were given in the last Annual Report.

On January 4, 1865, he observed the disappearance of ε *Piscium* at its occultation by the Moon. The result was negative as to any extensive atmosphere surrounding the Moon.

Mr. Huggins has analyzed the light of about 40 Nebulæ and clusters, in addition to those described in the last Report. All these objects give, with his telescope, either a continuous spectrum or a spectrum consisting of one, two, or three bright lines. These bright lines occupy the same positions in the spectrum as the bright lines of the nebulæ which he first examined. When the light of a Nebula is dispersed by the prism into a spectrum consisting of light of all refrangibilities, the spectrum is extremely faint. On this account it has not been possible to ascertain whether the continuous spectra, which some nebulæ give, are crossed with dark lines, as the solar and stellar spectra are.

The seven nebulæ which follow give a spectrum of one, two, or three bright lines. Some have in addition a faint continuous spectrum. These gaseous bodies are—

No. 2102	27 H. IV.	No. 4572	16 H. IV.	No. 4627	192 H. I.
4234	5 Σ.	4499	38 H. IV.		
4403	17 M.	4827	705 H. II.		

The following nebulæ and clusters have continuous spectra:—

No. 105	18 H. V.	No. 4159	1945 *h*.	No. 4485	56 M.
307	151 H. I.	4230	13 M.	4586	2081 *h*.
575	156 H. I.	4238	12 M.	4625	51 H. I.
1949	81 M.	4244	50 H. IV.	4627	192 H. I.
1950	82 M.	4256	10 M.	4600	15 H. V.
3572	51 M.	4315	14 M.	4760	107 H. II.
2841	43 H. V.	4357	190 H. II.	4815	53 H. I.
3474	63 M.	4437	11 M.	4821	233 H. II.
3636	3 M.	4441	47 H. I.	4879	251 H. II.
4058	215 H. I.	4473	Auw. N. 44.	4883	212 H. II.

Mr. Huggins has made an attempt to determine approxi-

matively the intrinsic brightness of three of the gaseous nebulæ. It is probable that these bodies consist of continuous masses of material. In the telescope they present surfaces subtending a considerable angle. As long as a distant object is of sensible size in the telescope its original brightness remains unaltered. By a suitable method of observation the intensities of these nebulæ have been obtained in terms of the light of a sperm candle burning at the rate of 158 grains per hour.

The Light of Nebula 4628 I H. IV. = $\frac{1}{1508}$th part of that of the Candle.
,, Annular Nebula in *Lyra* = $\frac{1}{6032}$th ,, ,,
,, *Dumbbell* Nebula = $\frac{1}{19604}$th ,, ,,

This estimation, in each case, refers to the brightest part of the nebula. Nebula 4628 gives a spectrum of three lines, and also a faint continuous spectrum. The nebula in *Lyra* and the *Dumbbell* nebula give one bright line only. These values are only approximative, and are too small by the unknown corrections for the possible power of extinction of space, and for the absorptive power of the Earth's atmosphere. Similar observations made at intervals of time may show whether the brightness of these strange bodies is undergoing increase, diminution, or periodic variation.

On January 9, 1866, Mr. Huggins observed the spectrum of Comet I. 1866. The comet appeared in his telescope as an oval nebulous mass, surrounding a small and dim nucleus. The prism showed that the nucleus was self-luminous, that it consisted of matter in the state of ignited gas, and that this matter is similar in constitution to the gaseous material of some of the nebulæ. The coma shines by light which has emanated from another source. Since the extremely diffuse matter of the coma cannot be supposed to contain solid or liquid matter at the high temperature necessary for incandescence, it seems almost certain that the coma reflects the Sun's light. On this supposition, the prism gives no information whether the material of the coma is solid, liquid, or gaseous. Terrestrial phenomena suggest a condition similar to fog or cloud. If the luminous gas of the nucleus suffers condensation and subsequent diffusion to form the tails of comets, it must pass through a condition in which it neither emits nor in any large degree reflects light. Dark spaces are frequently seen between the envelopes of comets.

The observations in full, of which the results are given in this Report, have been communicated recently in two papers to the Royal Society.

Mr. Birt continues his observations on the Moon's surface at Hartwell and London, as mentioned in the last Report.

During the past year four forms have been issued by the Lunar Committee of the British Association for the advancement of Science, viz. No. 1 for the reception of general observation of the Moon's surface; No. 2, a Table of areas for assisting in symbolizing objects; No. 3, for recording numerical and other data, appertaining to each object symbolised; and No. 4, for aiding in the computation of positions of the second order. These forms are found to be very useful, and by means of them nearly 800 objects have been symbolized and entered, and are now in progress of being inserted in an outline map of 100 inches to the Moon's diameter, on which the co-ordinates of each point determined by previous selenographers, especially Lohrmann, and Beer and Mädler, are being set off in parts of the Moon's semi-diameter to the fourth place of decimals and the excellent photograph taken by the President, Mr. Warren De la Rue, just after the lunar eclipse of October 4, 1865, and near the epoch of mean libration, is employed in the delineation of the forms of objects. As every principal object on the photograph will be transferred by measurement to the Map, it is expected that a degree of accuracy will be attained far beyond that which a filling in by eye-sketching can possibly accomplish. The most accurate determinations of lunar positions are those of the first order, but as these are comparatively few and the triangles formed by them large, amateurs might assist greatly by increasing their number. It is intended to lithograph portions of the Map now constructing and to distribute them to observers for this and kindred purposes.

Progress of Astronomy.

It is extremely difficult, if not impossible, to estimate correctly the amount of actual progress made in a science such as Astronomy during any single year. Not seldom the real work lies for the moment either not wholly matured, or by its very nature is to be stored away as seed for the harvest of future years. All that can be done in this direction is to give, so far as may be, a faithful record of what has actually come to the surface, and has already found a place in the scattered annals of learned Societies.

Cometic Structure.

Among the most remarkable and, until a year ago, certainly among the most unexpected accessions to our knowledge, is that which this year has come to us latest in the order of

discovery. We refer to Mr. Huggins' observations on the spectrum analysis of the light from a comet. The light of the nucleus of Comet I. 1866, as examined under his instrument, gives a spectrum consisting of but one bright line, whereas the spectrum formed by the light from the coma gives a spectrum which is continuous. The inevitable conclusion to be drawn from these observations is opposite to that which our prepossessions would have led us to expect, inasmuch as, consistently with the present state of our physical knowledge, we are forced to conclude that the light of the cometary nucleus examined by Mr. Huggins must have emanated from a gaseous source; whereas, guided partly by other physical considerations, no doubt remains that the coma contains fluid or solid materials. Thus the suspicion of analogy between cometic and nebular matter has received this further confirmation. No doubt difficult observations of this nature require repetition, but the known caution and experience of the observer invite our confidence.

Biela's Comet.

It is possible that in some unknown relation to the structure of these bodies thus revealed, may stand the fact of the non-apparition of Biela's dichotomized Comet, up to the present date, when the two bodies must have passed their perihelion. We are indebted to the labours of Mr. Hind and to the liberality of Mr. Bishop for the regular supply of ephemerides of this remarkable system, but hitherto with no results: had the comets been visible in Europe, apparently they must have been detected in the careful and extensive sweeps made with such instruments as the 15-inch object-glass at Pulkowa, and the 13-inch mirror at Mr. De La Rue's Observatory.

Hoek's Cometic Hypothesis.

Before dismissing the question of Comets, it seems desirable to call attention to two interesting papers written by Mr. Hoek, of Utrecht, on the subject of these bodies, and which have been inserted in our *Monthly Notices*. Mr. Hoek considers that he has advanced mathematical grounds whereon to found the probability that every star is associated with a cometary system of its own; but that, owing to the attraction of planetary or other cosmical matter, these bodies continually leave their proper primaries and revolve, either permanently in ellipses, or temporarily in parabolas or hyperbolas, round other Suns. Should this hypothesis of Mr. Hoek's be found, on examination, tenable, a new view of these erratic and intractable bodies will be opened.

Retardation of the Time of the Earth's Rotation.

A communication has been recently made by M. Delaunay to the Academy of Sciences at Paris, on the difficult question of the acceleration of the Moon's mean motion. M. Delaunay thinks that he can satisfactorily account for that outstanding part of the acceleration which at present appears not to be accounted for by planetary disturbance.

On the hypothesis that the disturbing forces of the Sun and Moon act on the lagging protuberance of the great tidal wave, he considers that the amount of this action is quite sufficient to produce a progressive augmentation in the time of the Earth's rotation on its axis, sufficient to account for the outstanding 6″ of the Moon's acceleration. Should this hypothesis eventually, and on re-examination, prove to be correct, it will be worthy of remark, that the scrupulously exact methods of astronomical investigation during the last few years, will have enabled us to estimate with greater accuracy two of the prime elements of the solar system, viz., the mean distance of the Sun from the Earth, and the length of the terrestrial day. Whether this be the case or not (and no opinion is here hazarded upon the subject), this remarkable conclusion of M. Delaunay can scarcely fail to give an additional impetus to the reconsideration of the more difficult and obscurer parts of the Lunar Theory.*

Variation of the Eccentricity of the Earth's Orbit.

In connexion, again, with the alteration in the eccentricity of the Earth's orbit, so intimately related to the former question raised by M. Delaunay, stand some remarkable speculations very recently raised or renewed by Mr. Croll relative to the alleged effects of climatal heat or cold produced in the course of many thousands of years by the variation in the aphelion and perihelion distances of the Sun from the Earth. Geologists give us ample evidence of at least one glacial period, and they are now beginning to observe indications of a succession of these periods of refrigeration, separated from each other by very long intervals of time. No doubt during the course of these immensely separated epochs there have been cycles of change in the eccentricity of the Earth's orbit, and it will be the province of mathematicians and physicists, possessing competent skill, to determine how far such a cause is sufficient to account for a succession of Glacial Periods.

* Since the above was in type, it is understood that the Astronomer Royal has investigated the effects of the Moon's action on the tidal wave, with results not in accordance with those of M. Delaunay. *Adhuc sub judice lis est.*

Solar Physics.

There has been much activity exhibited during the past twelve months in this interesting and important field of research, and more especially at Kew. The investigations there made, in addition to other results not yet published, confirm the hypothesis of the cavernous nature of solar spots, and of a downward rush of colder matter from above. In addition to the observations made at the Observatory itself, Messrs. De La Rue, Stewart, and Loewy, have availed themselves of all the drawings and the investigations on the subject within their reach. Especially they are measuring the areas of the spots on the solar surface, as depicted in Mr. Carrington's work, inasmuch as they conceive that the total amount of spot-area on the Sun at any assigned time, must be at least one important exponent of solar activity at the moment in question. In their confirmation of the truth of Wilson's hypothesis of the cavernous nature of solar spots, and in their own view of the downward rush, they are strengthened by the investigations of M. Chacornac, to whose successful labours on Solar Physics, astronomers are much indebted. The attention of observers will be amply repaid by a reference to the notices of M. Chacornac's labours published in the *Comptes Rendus* of the French Academy, in relation to the reflective power and the variable luminosity of the different portions of the photosphere of the Sun, and to the successive envelopes which appear to surround that body.

The Rev. J. Howlett, Mr. Lockyer, Professor Phillips of Oxford, and Canon Selwyn of Cambridge, have also made many valuable contributions in the same direction.

M. Faye, from the result of his investigations on the proper motions of Mr. Carrington's spots, comes to the same conclusion regarding the downward rush; but he dissents from the hypothesis of the Kew observers, that the deficient luminosity of a solar spot is owing to a diminution of temperature; on the other hand, it does not seem possible to harmonize this objection with the present state of our knowledge of radiant heat.

Again, M. Faye, in a Memoir recently communicated to the French Academy, on applying to the heliographic longitude of a Sun-spot the correction due to the parallax which would necessarily arise from its elevation above, or its depression beneath the surface of the photosphere, removes certain apparent anomalies which otherwise exist in the proper motions of the Sun-spots contained in Mr. Carrington's work. The following very interesting conclusions are deducible from M. Faye's mathematical investigation :—

1. Sun-spots are depressions beneath the surface of the Sun's photosphere, varying in depth from about $\frac{1}{100}$th to $\frac{1}{200}$th of the Sun's radius, i.e. from about 40,000 to 20,000 miles.

2. Many apparent *irregularities* in the proper motions of Sun-spots hitherto supposed to be capricious, or attributable to cyclones or tornados, or to their own mutual actions, are now probably explicable by the continued variation in the motion proper to each successive parallel of the photosphere.

3. The astonishing *regularity* in the motions of Sun-spots, the maintenance of which is thus demonstrated by M. Faye, appears to that astronomer incompatible with any hypothesis of mere superficial or local movements in the photosphere, but seems to point to some more general action arising from the internal mass of the Sun.

Professor Spoerer of Arlem in a memoir submitted to the Academy of Sciences in Berlin, has obtained a formula which (from four years' observations) expresses the law for the dependence of the period of the Sun's rotation on the latitude

$$\xi = 16^{\circ}{\cdot}8475 - 3^{\circ}\,3812 \sin (41^{\circ}\,13' + \text{Heliographic latitude.})$$

where ξ is the angle of rotation in a day.

M. Spoerer, though as it seems on insufficient data, calls in question Mr. Dawes's conclusion relative to the rotation of the darkest portion of a spot. Some observations of Padre Secchi appear to confirm the existence of those peculiar appearances on the general surface of the photosphere which were first described as willow-leaves by Mr. Nasmyth.

In addition to the establishment of a photoheliograph at Wilna, there is a prospect of the erection of a third at Quebec. If this hope is realized, there will then be a station in England, in Russia, and in America, by means of which, on account of the difference of longitude, we may hope to have an almost uninterrupted self-register of solar phenomena.

Telescopic Diameter of the Moon.

Some important calculations have been made from occultations of Stars by the Moon as observed at Greenwich and Cambridge, with a view of ascertaining the difference between the semi-diameters of the bright and the dark Moon. The result is that the Greenwich instruments give a telescopic semi-diameter too large by about $2''$. Mr. Airy considers it probable that the whole of these $2''$ is due to irradiation; and he remarks that, even if the whole of it were supposed to be caused by a lunar atmosphere, its attenuation must be so great that it would probably be discoverable by no other mode of observation. It may here be worthy of remark, that accurate measurements of Mr. De La Rue's photographs of the great Solar Eclipse of 1860 give precisely the same results; a circumstance which leads to the hope that photography may ultimately become a valuable auxiliary in even micrometrical observations.

Meteoric Astronomy.

Meteoric Astronomy is gradually being brought within the domains of known physical law. Regular observations, whenever the state of the atmosphere admits, are made at the Royal Observatory, at all the epochs of meteoric activity which have as yet been established or suspected. The well-known periodic star-shower of November was very diligently and successfully observed at Greenwich under Mr. Glaisher, at Cambridge under Prof. Adams, and at Hawkhurst by Mr. Alexander Herschel. Very complete preparations having been made at these three places of observation, the results possess unusual interest and claim our confidence. A more detailed account will be given in the *Monthly Notices* of next March.

Balloon Ascents by Night.

Some very remarkable results have been obtained from meteorological observations made by Mr. Glaisher during his intrepid ascents in balloons by night, which may have an important bearing both on the theory of astronomical refraction and on the theory of heat.

Mr. Glaisher observed that the decrease of temperature owing to increase of elevation, was variable throughout the day, but about sunset the temperature remained constant within the limit of an elevation of 2000 feet. This observation suggested the probability that after sunset the temperature might even increase with increase of elevation, at all events within the same limit of height. Hence arose the proposition for nocturnal ascents. Mr. Glaisher found that with the aid of a properly constructed Davy lamp he could read the instruments with sufficient facility, and in December last he made two ascents, and the results have justified the amount of trouble, and even of danger encountered in the enterprise. In the first ascent, when the sky was cloudless, the temperature *increased* with increase of elevation. During the second ascent, when the sky was overcast, there was a *small decrease* of temperature as the height increased.

In ascents by day, Mr. Glaisher found that the difference of the readings of two thermometers, the one having a blackened bulb exposed to the Sun, and the other shaded from the Sun, continued to *decrease*, as the elevation increased, until at the height of about five miles, when the readings of the two thermometers became identical. Mr. Glaisher considers that the nocturnal observations deserve repetition and extension.

Variable Stars.

Mr. Chambers has collected from various sources, and has published in the *Monthly Notices*, an interesting and valuable Catalogue of stars up to this date known to be Variable. It is herein that an ample field is thrown open to those non-professional astronomers who possess the requisite means at their private observatories. The alleged variations in the great nebula in *Orion*, and especially the anomalies in the visibility of some of the small stars in the trapezium recently indicated by Mr. Huggins, will furnish scope for employment of telescopes of various apertures.

Mr. J. Gurney Barclay has recently printed a volume of observations made at his Private Observatory, Leyton, during the years 1862–64, for which he merits the thanks of all persons interested in astronomy. Probably the observations of Minor Planets and Comets published in this Catalogue are unique as proceeding at this day from a private observatory. Mr. Barclay very handsomely acknowledges the assistance he had derived from M. Romberg, now attached to the Royal Observatory, Berlin.

Dr. Brünnow, lately of the Observatory Ann Arbor, U.S., has recently been appointed to succeed the late Sir W. Rowan Hamilton at Dublin, as Astronomer Royal for Ireland.

Papers read before the Society from February 1865 *to February* 1866.

1865.

Mar. 10. On the Spectrum of the Nebula in *Orion*. Prof. Secchi.
Observations of Comet I. 1864. Mr. Tebbutt.
Opposition of *Mars*, 1864. Mr. Joynson.
Drawings of *Mars'* Opposition, 1864. Mr. G. Williams.
Account of a Comet seen in Brazil. Prof. Challis.
On the Date of a Communication of a Mode of Observing Transits without Reference to Hearing or Touch. The Astronomer Royal.
Occultations Observed by him. Capt. Noble.
Comet Observations at Sydney Observatory. Mr. Smalley.

April 12. On the Planet *Mars*. Mr. Joynson.
On an Appearance presented by the Spots on *Mars*. Mr. Talmage.

Extract from Letter announcing Deaths of Professor Bond and Lieut. Gilliss. Dr. Gould.
On the Star *Baily Lalande* 14512 and Neb. H.N. 45. M. Schultz.
On a New Comet discovered in Australia. Mr. Ellery.
Ditto ditto Mr. Abbott.
Extract from Letter of Mr. Todd on the same Comet. Mr. Carrington.
Observation of ditto Mr. Abbott.
Ditto ditto Mr. Tebbutt.
Note on the Lunar Theory. Mr. Cayley.
On an Aluminium Bronze Transit Instrument. Col. Strange.
On the Perturbations of the Planet *Neptune.* Mr. Wackerbarth.

May 12. On the Notches or Bearings for the Pivots of Transits. Mr. Yeates.
On some peculiar Instances of Personal Equation in Zenith Observations. Mr. Dunkin.
Catalogue of Variable Stars. Mr. Chambers.
On the Comets, 1860, III., 1863, I., and 1863, IV. M. Hoek.
On the Photosphere of the Sun. Mr. Fletcher.
On a Double-Prism Eye-piece. Rev. W. R. Dawes.
On the Phenomena of Solar Spots. M. R. Wolf.
Discovery of a New Planet (*Beatrix*). M. De Gasparis.
Positions of Comet, 1865. Mr. Ellery.
Second Note on the Lunar Theory. Mr. Cayley.
Note on the Nebulous Star 45 H *IV. Geminorum.* Mr. Knott.
Constant of Lunar Parallax. Mr. Stone.

June 9. On an Artificial Horizon. Mr. Merrifield.
Observations of Comet, 1865. Mr. Tebbutt.
Observation of Lunar Eclipse of April 11, 1865. Mr. Freeman.
Note on ζ *Herculis.* Mr. Fletcher.
Description of an Equatoreal Mounting. Mr. Fletcher.
Description of an Aperture-diminishing Eye-piece and a Photometer of Neutral Tint Glass. Rev. W. R. Dawes.
Observations on the Sun's Photosphere. Mr. Lockyer.
On the Double Star δ *Cygni.* Mr. Knott.

Nov. 10. Note on an Error of Expression in two Memoirs of the Astronomer Royal on the Correction of the Elements of the Moon's Orbit. Mr. Airy.
Radiant Points of Shooting-Stars. Mr. A. Herschel.
Observations of Solar Eclipse, Oct. 19, 1865. Lord Wrottesley.
Places of Comet I. 1865. Mr. Ellery.

Observations of Encke's Comet. Mr. Tebbutt.
On the Comets of 1697 and 1683, 1860 III., 1863 I., and VI. M. Hoek.
On the Great Sun-Spot of October 1865. Rev. F. Howlett.
On the Solar Photosphere. Mr. Fletcher.
Observations on Solar Craters of September 28 and October 8, 1865. Mr. Brodie.
Occultations of Stars by the Moon. Capt. Noble.
On Photographs of Lunar Eclipse of October 1865. Mr. Brothers.
On Personal Equation in Reading Microscopes. Mr. Stone.
On the Telescopic Disks of Stars. Mr. Stone.
On the Physical Constitution of the Sun. M. Chacornac.
On Lambert's Theorem. Mr. Sylvester.
Elements of Comet I. 1865. Mr. Tebbutt.
Expressions for Plana's e, γ. Mr. Cayley.

Dec. 8. Observations of Comet III. 1860. M. Moesta.
Observations of Encke's Comet. Mr. Smalley.
On Prof. Krueger's Memoir on a star in *Perseus*. Mr. Carrington.
Note on ζ *Herculis*. Mr. Burr.
On the Solar Photosphere. Commander Ashe.
Geocentric N.P.D. of Moon and Moon-culminating Stars. Sir T. Maclear.
Mean R.A. and N.P.D. of Stars compared with Comet I. 1864. Sir T. Maclear.
Mean Places of Stars of Comparison, derived from Observations with the Transit Circle, and Apparent Places for Dates of Comparison with Comet I. 1865. Sir T. Maclear.
Observations of Solar Eclipse October 19, 1865. Rev. T. Chevallier.

1866.

Jan. 12. Proposition for a Telescope on the Andes. Commander Ashe.
On Sun-Spots. Mr. De La Rue and others.
Ephemeris and Elements of Comet of December 9, 1865. Dr. Donati.
Observations of η *Argûs*. Mr. Tebbutt.
On the Stars within the Trapezium of *Orion*. Mr. Huggins.
On the Diminution of Actinic Effect of the Sun near the Edge. Mr. Huggins.
Occultation of 115 *Tauri*. Mr. Talmage.
On a New Mode of Mounting Silvered Glass Specula and Diagonal Mirrors in Reflecting Telescopes. Mr. Browning.

List of Public Institutions and of Persons who have contributed to the Society's Library, &c. since the last Anniversary.

Her Majesty's Government.
The Lords Commissioners of the Admiralty.
Royal Society of London.
Royal Asiatic Society.
Royal Geographical Society.
Royal Institution.
Royal United Service Institution.
Geological Society.
Linnean Society.
Photographic Society.
Society of Arts.
British Meteorological Society.
British Association.
Art-Union of London.
Institute of Actuaries.
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Observatory, Harvard College.
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L'Académie Impériale des Sciences de l'Institut de France.
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Imperial Society, Cherbourg.
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Imperial Academy of Sciences, Vienna.
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The Society of Physics, Berlin.
Royal Academy of Sciences, Brussels.
Royal Academy of Sciences, Göttingen.
Royal Academy of Sciences, Munich.
Royal Academy of Sciences, Amsterdam.

Royal Society, Naples.
Royal Academy of Sciences, Madrid.
Royal Academy of Sciences, Bologna.
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Imperial Academy of Sciences, St. Petersburg.
Academy of Sciences, Batavia.
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M. Winnecke.
Prof. R. Wolf.
Dr. J. C. F. Zöllner.

ADDRESS

Delivered by the President, Warren De La Rue, Esq., on Presenting the Gold Medal of the Society to Professor J. C. Adams, Director of the Cambridge Observatory.

It may be truly said of the Theory of Gravitation that, while, on the one hand, its fundamental laws are easily apprehended by the human intellect, on the other the results which the application of this theory produces are of so intricate a nature, especially when the mutual action of more than two bodies is concerned, that it requires the exercise of the highest faculties of minds specially cultivated, to trace out all the consequences which arise out of the mutual attractions of the particles and masses of matter which constitute the Universe.

No application of the laws of gravitation has given more trouble to mathematicians than the development of the Lunar Theory; frequently, indeed, in the history of physical astronomy grave doubts have been entertained respecting the possibility of accounting, by means of the grand and simple law discovered by Newton, for the perplexing inequalities to which the Moon's motion is subject, and which observation has shown to exist; and even the results of observations themselves have been called into question, when the mathematician has failed to account for the phenomena they disclose.

It is for most valuable contributions to the development of this Lunar Theory that your Council have awarded our Medal to Professor Adams. While it is extremely gratifying to me to be the medium of explaining the grounds on which this award has been made; at the same time I am deeply conscious that it would have been far better, in the interest of science, if the award had been made under the presidency of a physical astronomer specially qualified by his previous pursuits to do full justice to the works of the illustrious mathematician whose merits we desire, this day, to recognise.

The Medal has been awarded to Professor Adams for his

investigations in respect of the Lunar Parallax and the Secular Acceleration of the Moon's mean motion.

I propose first to take into consideration the last named of these works, which was communicated to the Royal Society so far back as June 1853, and published in the *Philosophical Transactions*, vol. cxliii. part iii. page 397. It is entitled, "On the Secular Variation of the Moon's Mean Motion." No contribution to astronomical physics has given rise to more discussion in modern times than the investigation of this problem by Professor Adams; and it will be in the recollection of most now present that our *Monthly Notices* from 1858 to 1862 contain much controversial writing in relation to this subject, in which several of the most renowned physical astronomers of the day took part. A summary of the history of this famous problem is given with remarkable clearness by M. Delaunay in the *Additions à la Connaissance des Temps* for 1864;* and although I shall borrow from that paper in the course of this discourse, I would recommend the perusal of the original to all who may be desirous of acquainting themselves fully with the subject under consideration.

Let me recall to your recollection that in dealing with the perturbations caused by the Sun in the Moon's motion, we have to consider only the differences between the action of the Sun on the Moon and on the Earth. If both bodies were attracted equally and in parallel directions, then their relative motions would not be disturbed, and would be in harmony with the laws of elliptical motion as derived from the force of gravitation. But the distance of the Sun from the Moon and Earth, although great, is not so large but that the Moon is, in the course of her orbital motion round the Earth, sensibly nearer to the Sun at one time than at another; moreover, the angles which are formed by the line joining the Sun and Moon's centres with the line joining the Sun and Earth's centres, are sensible, though small. It results therefrom that the attractions of the Sun on the Moon and on the Earth are generally unequal, and act in different directions; and thus arises a disturbing action of the Sun, causing sometimes an increase, sometimes a decrease of the Moon's gravitation towards the Earth; sometimes a retardation and sometimes an acceleration of the areal velocity; also continual changes in the excentricity and inclination of the Moon's orbit, and in the position of its perigee and nodes; indeed, producing such a perplexity of inequalities that they can only be traced to their final result by discussing the effects of the disturbing forces with all the minuteness of which the resources of analysis permit. It is in the investigation of these various perturbations that the famous problem of three bodies consists, "the rigorous solution of which," says Laplace,†

* Pages 21 to 72 inclusive.

† *Exposition du Système du Monde*, 5th Edit. vol. ii., p. 65.

"surpasses the resources of analysis; but which fortunately admits of being resolved by approximation, in consequence of certain favourable conditions in the problem, for example, the proximity of the Moon to the Earth in relation to her distance from the Sun." He goes on to say that the most careful consideration is required to disentangle those terms the influence of which is sensible, and to determine with exactness those which, although small in themselves, acquire in the successive integrations a sensible value. It will be seen hereinafter that mathematicians of the highest order are liable to overlook some of these terms, and that consequently no physical astronomer ought to allow himself to be in the least deterred from re-investigating a problem because it has passed under the consideration of men whose names are the greatest in science.

In further elucidation of this reflection, I quote the following remarks of M. Delaunay,* who says, "When the extreme complication of the questions comprehended in Celestial Mechanics is taken into consideration, it will be understood that things do not present themselves to all minds with the clearness which obtains in elementary algebra. By reason of this complication, problems which are intended to be resolved are not attacked at once in their entirety; but divers portions of the solution are successively sought for in such a manner that the complete solution may be subsequently obtained by uniting together the several parts thus separately determined. In consequence of this manner of operating, the greater number of the equations which are taken into consideration are recorded in an incomplete form; only those terms are retained which are considered to have some influence on the partial results sought for, all the other terms being unexpressed; some of these unexpressed terms may however have a much greater influence on the *final solution* of the problem than those terms which are retained. The talent of the mathematician who undertakes the discussion of such questions consists precisely in distinguishing, among the excessively numerous terms of which every equation would consist if stated at length, those which ought to be retained as having some influence on the partial results to be obtained. It will therefore be understood that a discussion may arise on the influence or non-influence of certain terms on such and such a part of the general solution, and consequently on the retention or rejection of those terms in the investigation of a part of the solution; all possibility of such a discussion would disappear if the complete solution of the question could be at once and fully undertaken, but this in the greater number of cases must be considered as beyond the power of the human mind."

I have been induced to quote from Laplace and Delaunay their opinions in regard to the difficulties of the analysis, be-

* *Additions à la Conn. des Temps*, 1864, pp. 51, 52.

cause I am sure that during the controversy on the secular acceleration of the Moon's mean motion many persons were perplexed why any difference of opinion should arise respecting a question which in reality resolved itself into one of pure mathematics.

To return to the secular acceleration of the Moon's mean motion:—you will remember that this inequality was discovered by comparing records of ancient eclipses with observation long before theory could account for it. Halley was the first who suspected its existence; he found, on calculating the Moon's place for the epochs of certain ancient eclipses recorded by Ptolemy, by means of her mean motion computed from modern observation, that the result was such as to indicate a position in her orbit less advanced than her recorded place, and the time of an eclipse so calculated was later than that actually recorded. He inferred therefore that the angular motion of the Moon must have been accelerated since the earliest astronomical records. Halley first alluded to this phenomenon in 1693, and nearly sixty years elapsed before this suspicion of his was confirmed. Dunthorne in 1749 communicated a paper to the Royal Society which contained a discussion of all the observations calculated to throw light upon the subject; Mayer also arrived at the conclusion that the Moon's motion had continually been accelerated from the earliest records. Both these astronomers found that the same mean motion of the Moon could not satisfy both modern observations and the records of the eclipses observed by the Chaldeans and Arabs. They attempted to represent these by adding to the mean longitude of the Moon a quantity proportional to the square of the number of centuries before or after 1700. According to Dunthorne this quantity should be 10″ for the first century; Mayer made it 6″·7 in his first lunar tables,* and subsequently 9″ in his later tables. Lalande also arrived at the same result as Dunthorne, namely, 9″·886, which he subsequently fixed at 10″ from the year 1700. Bouvard and Burg also, by discussing a great number of observations in the two preceding centuries, determined the acceleration with great accuracy and confirmed the foregoing results.

Now the lunar acceleration having been incontestably established, it was an object of great interest to ascertain the cause of this inequality. Euler came to the conclusion that it could not be produced by the force of gravitation. Lagrange at a later period demonstrated that neither the figure of the Moon, nor that of the Earth, nor the direct attractions of the planets, could be the cause of the phenomenon.

To give an idea of the position of the question when it was

* It is generally stated that Mayer employed in his Tables the coefficient 7″ for the lunar acceleration, but M. Delaunay informs me that 6″·7 was really the co-efficient used by Mayer.

See *Add. à la Conn. des Temps*, 1864, p. 26.

attacked by Laplace, I may quote from his *Exposition du Système du Monde*,* the following remarks, slightly paraphrased:—"This subject has much occupied the attention of geometers, but their researches were for a long time unfruitful, not having led to the discovery of any cause which could alter the Moon's mean motion, whether by the action of the Sun and planets on the Moon or in consequence of the non-spherical figures of the Moon and the Earth; some astronomers have in consequence come to the conclusion that the secular acceleration had no existence; others, in order to explain it, had recourse to divers hypotheses, for instance, the action of comets, the resistance of the ether, and the successive transmission of the force of gravitation. But the accordance of other celestial phenomena with the theory of gravitation is so perfect, that it could but be seen with regret that the secular acceleration of the Moon should refuse allegiance to this theory, and thus alone constitute an exception to a simple and general law which by the magnitude and variety of the objects which it embraces does so much honour to the human intellect."

After many unsuccessful attempts to account for this phenomenon, Laplace at last succeeded in mastering the difficult problem, which had baffled so many distinguished mathematicians and had escaped the sagacity of Lagrange, who (says M. Delaunay†) "had almost touched it with his finger in his investigation of the *Secular Variations of the Mean Motion of the Planets*" inserted in the *Memoirs of the Academy of Berlin*, 1783.

Laplace communicated his discovery to the *Academie des Sciences* of Paris on the 19th November, 1787. In the *Exposition du Système du Monde*‡ he thus states the cause of the secular acceleration:—"*The secular equation of the Moon is due to the action of the Sun on this satellite, combined with the secular variation of the excentricity of the terrestrial orbit.*"

In accounting for the secular acceleration of the Moon, Laplace only took into account directly, the *radial* component of the disturbing action of the Sun, which, as you know, tends, on the whole, to dilate the lunar orbit. This force, it will be remembered, does not affect the areal velocity of the Moon, but it does the angular velocity. Its effect is greater when the Earth is in perihelion than when it is in aphelion, so that the Moon's orbit is more dilated when the Earth is in perihelion; it follows that, the description of the areas remaining the same so far as this central disturbing force is concerned, the angular velocity is less and the lunar month is consequently longer in our winter that in our summer. This

* Fifth Edition, vol. ii., pp. 75, 76.
† *Addit. à la Conn. des Temps*, 1864, p. 39.
‡ Fifth Edition, vol. ii., p. 76.

alternate dilatation and contraction of the lunar orbit, as you know, gives rise to the *annual equation*. The mean diminution of the Moon's angular velocity, caused by the radial disturbing force, depends, in some degree, on the excentricity of the Earth's orbit, and is greater or less according as that excentricity is greater or less. Hence if the excentricity of the Earth's orbit diminishes, the Moon's angular velocity will increase.

Now among the changes in the elements of the Earth's orbit produced by the attraction of the planets, there occurs this change of excentricity, which is the cause of the lunar acceleration; the semi-major axis of the Earth's orbit, or the Earth's mean distance, undergoes no permanent alteration, but the excentricity of the Earth's orbit is always varying; for ages it has been gradually becoming less, and according to Leverrier* will continue to do so for about 23,980 years from 1839, when it will have attained a minimum value of 0·003314, after which it will again increase, until it attains its maximum.

As the excentricity of the Earth's orbit becomes less each succeeding year the Earth and Moon will not approach so close to the Sun at the epoch of perihelion as in the preceding year, and the Moon's orbit will gradually contract, and consequently her angular motion will increase. Laplace says, "The action of the Sun on the Moon diminishes by $\frac{1}{179}$th her angular velocity, and the numerical coefficient of this diminution of the angular velocity varies inversely as the cube of the distance of the Sun and Earth. Now in developing the inverse cubic power of the distance in a series arranged with regard to the sines and cosines of the mean motion of the Earth and its multiples, the semi-major axis of the orbit being taken as unity, it is found that this series contains a term equal to three times the half of the square of the excentricity of the Earth's orbit; the diminution of the angular velocity of the Moon contains the product of this term by $\frac{1}{179}$th of this velocity. This product would be confounded with the mean angular velocity of the Moon, if the excentricity were constant; but its variation, though small, has an influence in the long run on the lunar motion. It is evident that it accelerates this motion when the excentricity diminishes." Ultimately this acceleration will in the course of ages cease, and the Moon's mean angular motion will then begin to decrease.

Laplace further pointed out that the motions of the lunar perigee and nodes were subject to secular inequalities dependent on the same cause as the lunar acceleration. He also proved that the Moon's orbit has always the same mean inclination to the ecliptic, and that the changes produced in the excentricity and major axis of the lunar orbit, by the change of excentricity of the Earth's orbit, are insensible.

* *Additions à la Connaissance des Temps*, 1843, p. 47.

Laplace found that the lunar acceleration amounted to $11''{\cdot}135$ in a century, though, in consequence of taking into account a modification of the masses of *Mars* and *Venus*, upon which the rate of decrease of excentricity of the Earth's orbit in a great measure depends, he subsequently reduced this quantity to $10''{\cdot}18$. But in carrying back the calculations of the Moon's place to the period of the Chaldean observations he found that it was necessary to introduce a new term depending on the cube of the time,* so that if t denoted the number of centuries from the assumed epoch, the acceleration would be

$$10''{\cdot}1816\, t^2 + 0''{\cdot}01854\, t^3.$$

The value assigned by Laplace for the lunar acceleration accorded fairly † with ancient eclipses and modern observation, and it is a fortunate circumstance that, at the period it was published, it did so, otherwise grave doubts might have arisen as to the sufficiency of the theory of gravitation to account for the phenomenon then under anxious consideration.

Subsequently in 1820, in consequence of the powerful impulse of the *Académie des Sciences* of Paris,‡ the theory of the Moon was again investigated by M. Damoiseau, and also by MM. Plana and Carlini. They carried their approximations to a much higher order than Laplace had done, but they failed to discover that the tangential force also produced a permanent secular effect. Their investigations did not greatly change the coefficient arrived at by Laplace ($10''{\cdot}1816$). Plana's value was $10''{\cdot}58$ in a century, whereas M. Damoiseau, by calculating directly this coefficient in a numerical form, arrived at the number $10''{\cdot}72$.

These new values for the secular acceleration of the mean motion of the Moon did not materially alter the times computed for the ancient eclipses, so that Laplace rested perfectly satisfied with the results of analysis, and was not induced therefore to investigate rigorously all the effects which would be produced by the Sun's disturbing force. With respect to the theory which ascribed the acceleration of the Moon's mean motion to the resistance of the ether in space or to the progressive transmission of the force of gravitation, he thus disposes of these hypotheses: "When only the accceleration of the mean motion of the Moon was known, it might have been attributed to the resistance of the ether, or to the successive transmission of the force of gravitation. But analysis shows us that these two causes cannot produce a sensible alteration in the mean mo

* *Exposition du Système du Monde*, 5th edit., vol. ii., p. 76.

† The accordance was not perfect, as will be seen by an examination of the Table inserted by M. Delaunay in the *Add. à la Conn. des Temps*, 1864, p. 29.

‡ At the instigation of Laplace the Academy of Sciences of Paris made the Lunar Theory the subject for competition for the Grand Prize of Mathematics.

tions of the nodes and perigee which I have shown to depend on the same cause as the secular acceleration."

He goes on to say that the accordance between theory and observation in respect of the secular acceleration establishes with certainty the *constancy of the length of the day*; "for if the length of the day were greater now by the one-hundredth of a second than in the days of Hipparchus, the actual duration of a century would be greater by 365·25 seconds, during which time the Moon describes an arc of 173″·2: the mean secular acceleration of the Moon would in consequence be increased by 4″·38 for the first century, from the year 1801; a quantity not admissible by theory." Of course if the accordance between theory and observation, thus supposed by Laplace, does not exist, this reasoning of his falls to the ground; and it will presently be seen that M. Delaunay has recently from other considerations arrived at the conclusion that the sidereal day has actually lengthened.*

M. Hansen was the next to take up the question, he calculated the coefficient of the secular acceleration of the Moon on several occasions, and found in the first instance 11″·93 (*Astronomische Nachrichten*, No. 443, March 1842), which he afterwards reduced to 11″·47 (No. 597, May 1847).

Such was the state of the theory when Mr. Adams, in a Memoir read before the Royal Society of London on the 16th of June, 1853, and published in the *Philosophical Transactions* for the same year, pointed out an important error in the method pursued by MM. Plana and Damoiseau in obtaining the secular acceleration of the Moon. The correction of this error involved a considerable diminution in the theoretical value of this acceleration. The object which Mr. Adams proposed to himself in this Memoir was to determine the amount of the acceleration of the Moon's mean motion which was due to the cause assigned by Laplace. The problem which he undertook to solve may be accordingly stated in the following purely mathematical form. If the Earth be supposed to describe about the Sun an elliptic orbit, the excentricity of which varies slowly and proportionally to the time, while the other elements remain constant, what will be the change in the Moon's mean motion due to this change of excentricity of the Earth's orbit.

In the introduction to his Memoir, Mr. Adams thus briefly explains the principle of the method employed by Laplace in the solution of this problem:—

"It is readily shown that the mean central disturbing force of the Sun, by which the Moon's gravity towards the Earth is diminished, depends not only on the Sun's mean distance, but also, in some degree, on the excentricity of the Earth's orbit. Now this excentricity is at present and for many ages has been diminishing, while the mean distance re-

* See page 174.

mains unchanged. In consequence of this, the mean disturbing force of the Sun is also diminishing, and therefore the Moon's gravity towards the Earth at any given distance is, on the whole, increasing. Also the area described by the Moon about the Earth is not affected by this alteration of the central force, whence it may be readily inferred that the Moon's mean distance from the Earth will be diminished in the same ratio as the force at a given distance is increased, and that the mean angular motion will be increased in double the same ratio."

In the *Mécanique Céleste*, the approximation to the value of the acceleration is confined to the principal term, which depends on the first power of the Sun's disturbing force.

In the theories of Damoiseau and Plana, a great extension is given to the theory of Laplace by taking into account the square and higher powers of the disturbing force, but the principle of the methods by which they determine the value of the acceleration, may be regarded as essentially the same as that of Laplace's method, which has just been explained.

Now, it will be observed that the reasoning employed in this method is founded on the supposition, that the area described in a given time by the Moon about the Earth undergoes no permanent alteration, or, in other words, that the tangential disturbing force produces no permanent effect. If we confine our attention to the first power of the Sun's disturbing force, as Laplace has done, this supposition holds good, but if we proceed to take into account the square and higher powers of the disturbing force, the same supposition is no longer strictly true.

In Mr. Adams' Memoir he endeavours to point out, in popular language, the manner in which the inequalities of the Moon's motion are modified by a gradual change of the mean central disturbing force, so as to give rise to a permanent alteration of the mean areal velocity of the Moon about the Earth.

As an example, he takes the inequality called the *variation*, which is the most direct effect of the disturbing force.

In the ordinary theory, the orbit of the Moon, as affected by this inequality only, would be symmetrical with respect to the line of conjunction with the Sun, and the areal velocity generated while the Moon was moving from quadrature to syzygy would be exactly destroyed while it was moving from syzygy to quadrature, so that no permanent alteration of areal velocity would be produced.

In reality, however, the magnitude of the disturbing force by which this inequality is caused depends in some degree on the excentricity of the Earth's orbit, and as this is continually diminishing, the central disturbing forces at equal angular distances on opposite sides of conjunction will not be exactly equal. Hence the orbit will no longer be symmetrically situated with respect to the line of conjunction. Now the

change of areal velocity produced by the tangential force at any point depends partly on the value of the radius vector at that point, and consequently the effects of the tangential force before and after conjunction will no longer exactly balance each other.

The other inequalities of the Moon's motion are similarly modified, especially those which depend more directly on the excentricity of the Earth's orbit, so that each of them gives rise to an uncompensated change of the areal velocity.

The distortion in the form of the Moon's orbit, which is thus produced by the continual alteration in the excentricity of the Earth's orbit, is of the order of the Sun's disturbing force. Hence the alteration of the tangential disturbing force due to this distortion will be of the order of the square of the Sun's disturbing force. And, since this alteration of the tangential force is the only cause which produces a permanent change in the Moon's areal velocity, it follows that the rate of change of the mean areal velocity will also be of the order of the square of the Sun's disturbing force.

It is evident that this change of the Moon's mean areal velocity will give rise to a change of the same order in the amount of the lunar acceleration.

After giving this general reasoning in the introduction to his Memoir, Mr. Adams proceeds to the strict mathematical treatment of the problem.

For the sake of simplification he confines his attention to the terms which are independent of the excentricity and inclination of the Moon's orbit, and which do not involve any higher power of m than the fourth — m denoting, as usual, the ratio of the Sun's mean motion to that of the Moon.

He arrives at the conclusion that instead of the secular equation contained in Plana's expression for the true longitude in terms of the mean, namely,

$$-\left(\tfrac{3}{2}m^2-\tfrac{2187}{128}m^4\right)\int(e'^2-\mathrm{E}'^2)\,n\,dt$$

the following secular equation should be substituted

$$-\left(\tfrac{3}{2}m^2-\tfrac{7542}{128}m^4\right)\int(e'^2-\mathrm{E}'^2)\,n\,dt$$

The principal term of the correction to be applied to Plana's value of the secular acceleration is therefore

$$\tfrac{5355}{128}m^4\int(e'^2-\mathrm{E}'^2)\,n\,dt$$

And as the value of the integral

$$\int (e'^2 - E'^2)\, n\, dt = -\,1270''\left(\frac{t}{100}\right)^2 \text{ nearly}$$

where t is expressed in years, the numerical value of this term is

$$-\,1''{\cdot}66\left(\frac{t}{100}\right)^2$$

Of course it might be expected that the succeeding terms of Plana's expression for the secular equation would also be materially changed.

It was not till 1856 that this important Memoir of Professor Adams' attracted attention, when M. Plana himself was induced by it to re-examine a portion of his *Théorie du Mouvement de la Lune*. In a paper published in April of that year, he admitted that his theory was imperfect, and he deduced Mr. Adams' result from his own equations. Soon afterwards, however, M. Plana retracted his admission of the correctness of Mr. Adams' result, and obtained another result, differing both from that which he had originally found, and also from that of Mr. Adams.

In 1857 Professor Hansen's valuable work, entitled *Tables de la Lune*, was published. In this work the coefficient $12''{\cdot}18$ was adopted for the secular acceleration of the Moon's mean motion.

M. Delaunay in 1859 undertook the investigation of this part of the lunar theory, and in the first instance carrying the calculation as far as the term involving m^4, he found exactly the same value for this term as Mr. Adams had done, namely $\frac{3771}{64}\, m^4$. This result was communicated to the Academy of Sciences on the 17th January, 1859. On being informed of this, Mr. Adams immediately published the values which he had some time previously obtained for the terms involving m^5, m^6, and m^7 (*Académie des Sciences de Paris*, 31 January, 1859, *Monthly Notices*, April 8, 1859*). (This paper contained an erroneous coefficient, which was subsequently corrected. It arose from writing $\frac{3372845}{10368}\, m^5$, instead of $\frac{3572845}{10368}\, m^5$, and was therefore simply a clerical error.) The result of Mr. Adams' new investigation was to reduce the coefficient of the lunar acceleration to $5''{\cdot}7$, or about half the value hitherto received. In a supplement to this paper† Mr. Adams communicated the values of the two principal terms in the expression of the lunar acceleration, which depend on the excentricity and inclination of the orbit. He found that the

* *Monthly Notices*, vol. xix., p. 207.

† Ibid. p. 208.

term in e^2 increases the coefficient of the acceleration by $0''{\cdot}036$, while the term in γ^2 diminishes it by 0·097, so that this coefficient is ultimately reduced to $5''{\cdot}64$. I call attention here to a remark of Mr. Adams, which was expanded in a subsequent communication into a fuller anticipation, that another cause besides gravitation was concerned in producing the lunar acceleration. He says that he believes the value just given to be within one-tenth of a second of the true *theoretical* value of the coefficient of the secular accceleration, and adds, "Whether ancient observations admit of such a small value of the acceleration is a different question." M. Delaunay, on the 25th April, 1859, communicated to the *Académie des Sciences* the result of a more elaborate investigation, wherein he confirmed all the new terms of Prof. Adams just alluded to, and, by carrying the approximation to the eighth order, fixed the lunar acceleration at $6''{\cdot}11$ in a century.

It was after the publication of the foregoing results that the controversy commenced which occupied so much of the attention of physical astronomers. The propriety of introducing the new terms developed by Mr. Adams was called into question by M. de Pontécoulant (*Académie des Sciences*, 30 May, 1859), and, under date of the 28th of the same month, this distinguished mathematician communicated a note on the same subject to the then President of our Society (Rev. R. Main)*. In a letter from Prof. Hansen† to the Astronomer Royal, May 21, 1859, attention is called to those three values at which he had arrived at different periods, and which I have before enumerated (see pages 164 and 167). With the solicitude which he has invariably evinced for the interests of astronomy, Mr. Main undertook the examination of the question, and communicated to the Society‡ an elaborate paper "On the Present State of the Controversy respecting the amount of the Acceleration of the Moon's Mean Motion," wherein he clearly and fairly stated the bearings of the points under dispute, and thus narrowed the field of controversy—"It is incumbent on the opponents of Mr. Adams to show clearly that the tangential force, producing alterations of the areal velocity, can produce no effect on the secular acceleration." He at that early stage stated that "Adams and Delaunay seem to have right on their side." "But if we accept Mr. Adams's value of the coefficient, and the principles of his investigation be established beyond controversy, as in my own mind I have very little doubt they will be, this value is far too small to satisfy the ancient eclipses, and therefore some other cause (such as a resisting medium) totally different from the disturbing influences

* *Monthly Notices*, vol. xix., p. 235.
† Ibid. p. 236.
‡ Ibid p. 268.

of the Sun and planets, must be resorted to, or we must hope, from a hint dropped by M. Delaunay at the end of his paper, that he has some means, at present kept out of sight, for laying the ghost he has helped to raise."

In the *Monthly Notices* for April 1860, vol. xx. p. 225, Mr. Adams published his "Reply to various objections" which have been brought against the theory of the secular variation of the Moon's mean motion.*

In this paper he calls attention to the fact that the question under consideration is a purely mathematical one, with the decision of which *observation* has nothing whatever to do.

If, as seems probable, ancient observations should show that the Secular Variation of the Moon's Mean Motion is different from that which, according to theory, would be produced by the known change of the excentricity of the Earth's orbit, it would be necessary to draw the conclusion that *the mean motion of the Moon is affected by some other cause or causes, besides the variation of the excentricity which has been taken into account.* Mr. Adams remarks that "this fact, if established, would be a most interesting one, and might put us on the traces of an important physical discovery."

It has been already mentioned, at the outset of these remarks, that the object of Mr. Adams' investigations was to find the effect on the Moon's motion of a slow, uniform change in the excentricity of the Earth's orbit, while the other elements of that orbit were supposed to remain constant.

Of course, in nature, the change of excentricity of the Earth's orbit cannot take place without being accompanied by changes in the other elements of the orbit, and in addition to the secular change of excentricity there will be other changes which are periodic.

If any part of the lunar acceleration could arise from such changes of the elements as those which have just been mentioned, of course this part would not be given by Mr. Adams' theory, which only professes to determine what amount of acceleration is due to the regularly progressive diminution in the excentricity of the Earth's orbit. If it should turn out to be true that the acceleration is partly due to such periodic changes in the elements of the Earth's orbit, this would in no way be inconsistent with Mr. Adams' result.

In the paper last cited, Mr. Adams points out that he has made *no assumption* in his Memoir of 1853 respecting the variability of the Moon's mean areal velocity. He *proves* mathematically that this areal velocity does vary and finds the amount of its variation, and the general reasoning given in the introduction is simply the translation, so to speak, of his analysis into ordinary language, in order to make the nature of his correction to Plana's theory more generally intelligible.

* See Appendix A.

He remarks, too, that even if he had started with the assumption that the mean areal velocity was variable, no error could have been caused thereby, for if this areal velocity had been really constant, he would have simply found its variation equal to zero. In mathematical language, the terms *constant* and *variable* are not looked upon as exclusive of each other, but a *constant* is regarded as a particular case of a *variable* quantity.

Mr. Adams also states that in his investigations made subsequently to his Memoir of 1853, he employed a new method in which his results are obtained without taking into consideration the mean areal velocity at all.

It was suggested by M. Hansen, that the difference between his value of the secular acceleration and that obtained by M. Delaunay and Mr. Adams, might arise from a want of convergency in the series proceeding according to powers of m, by means of which they determine the coefficient of the acceleration.

In order to remove all possible objection, Mr. Adams calculated the value of that part of the coefficient of the acceleration which is independent of the excentricity and inclination of the Moon's orbit, by a method which does not require any expansion in powers of m, and the resulting coefficient exactly agreed with that which he had previously found by means of the series.

It would be needless to reproduce here the various arguments which were used to show that Mr. Adams was wrong in taking into consideration the variability of the areal velocity of the Moon, or to make any further reference to the objections brought against the principles on which he had conducted his investigations, because ultimately his results, and consequently those of M. Delaunay, were confirmed by the independent investigations of several mathematicians. For example, by the late Sir John Lubbock,* who used in this investigation formulæ which he had before employed in recalculating many of the inequalities of the Moon's motion. This distinguished mathematician, who contributed so greatly to the development of the lunar theory, arrived at precisely the same value for the much-contested term in m^4 as Mr. Adams had done in 1853.

Professor Donkin, in a communication read June 14th, 1861, and published in the *Monthly Notices*,† gave the results of an investigation of the coefficient of m^4, the calculation of which involves the whole mathematical question of the subject then under dispute. He used the same method of the variations of elements which had been employed by M. Delaunay; but as he

* *Mem. Ast. Soc.* vol. xxx. pp. 38–52, "On the Lunar Theory," read November 9, 1860.

† Vol. xxi. p. 221.

had not had the opportunity of seeing what M. Delaunay had published, his result may be regarded as independent testimony of the correctness of Mr. Adams' value for that term.

Professor Cayley,* by following a totally different method from any used previously, also undertook the calculation of the coefficient of m^4, and completely confirmed Mr. Adams' value.

M. Delaunay had previously, by two entirely different methods, also arrived at the same value as Mr. Adams.

Baron Plana had, in 1856, as I have before stated, recognised the correctness of Mr. Adams' value for the coefficient of m^4; but subsequently, in a paper read before the Academy of Sciences of Turin, in June 1856, he retracted that admission, having arrived at a new result by a subsequent investigation. Ultimately, in 1860, after repeating his doubts as to the correctness of Mr. Adams' coefficient for m^4, he,† in a correspondence with Sir John Lubbock, admitted the correctness of Mr. Adams' value.

In these numerous confirmations of the correctness of the result of Professor Adams, the verifications were obtained by different methods, in which the *savants* who employed them did not start with the assumption either of a variability or of a non-variability of the areal velocity; and the calculations were consequently made without regard to the areal velocity at all, the variability or non-variability of the mean areal velocity being in fact part of the problem to be determined.

After the thorough investigation which the problem of the secular equation of the Moon's motion has received at the hands of so many distinguished mathematicians, there can no longer be any question as to the correctness of the value of the coefficient determined by Mr. Adams and M. Delaunay. But here comes a difficulty which, however, in no way casts any doubt on Mr. Adams' theory: it is this, that the coefficient $5''{\cdot}7$, or $6''{\cdot}11$, as M. Delaunay has made it by further developing the series, does not account for the amount of the secular acceleration as shown by observation. It is perfectly true, notwithstanding some uncertainty as to the actual date and place of the ancient solar eclipses, and also as to the correctness of the records of certain lunar eclipses, that Prof. Hansen's value of the secular acceleration, namely $12''{\cdot}18$, accords better with these phenomena than the smaller number $6''$.

Mr. Hartwig, in the *Astronomische Nachrichten*, No. 1241,‡ gives the results of his calculations of Nineteen Lunar Eclipses contained in the *Almagest*, by means of Hansen's Solar and Lunar Tables.

He comes to the conclusion that the results, on the whole, accord satisfactorily with Hansen's Tables.

* Abstract, *Monthly Notices*, vol. xxii. p. 32; Paper in full, ib. p. 173.

† See *Add. à la Conn. des Temps*, 1864, p. 56.

‡ 1860, p. 257. See Appendix B.

In the *Berichte der K. Sächs. Gesellschaft zu Leipzig*, 15 April, 1863, is contained a short paper by Prof. Hansen, in which he states that as the value of the acceleration, as calculated by Adams and Delaunay, is considerably—nearly 6″—less than that employed in his Lunar Tables, such smaller value (as he had shown in the *Comptes Rendus*, t. l., 1860·0) cannot represent the ancient eclipses.* Assuming Adams and Delaunay's coefficient to be the correct acceleration due to the change in the excentricity of the Earth's orbit, the deficiency must be accounted for by other causes. That which most naturally presents itself is a change in the length of the sidereal day or period of the Earth's rotation; and he remarks that Laplace's demonstration of the invariability of the sidereal day depends on the very assumption that the acceleration is *wholly* accounted for by the change of the excentricity of the Earth's orbit, and consequently must in the present question be put aside.† He proceeds to show that the deficiency of 6″ in the acceleration would be accounted for by the almost infinitesimal increase of $0^{s}\cdot01197$ in the length of the sidereal day in the course of 2000 years.‡ He, however, goes on to say that it is remarkable that the value of the acceleration as obtained by himself should agree so well with observations without the assumption of any such supplemental cause.

In the second part of Hansen's *Darlegung der theoretischen Berechnung, &c.*, which has recently appeared, he has recalculated the secular variation of the mean longitude, and has found the coefficient 12″·557. It has been pointed out to me, however, that in his investigation he appears to have fallen into an error of the very same nature as that of Plana and Damoiseau to which I have already alluded. It is true that in the passage in Art. 219 Prof. Hansen professes to have already taken account of the terms to which Mr. Adams called the attention of astronomers, but notwithstanding this it would appear that he entirely neglects the non-periodic term which the terms referred to will introduce into the expression of the Sun's tangential disturbing force.

In fact, in the paragraph commencing at the foot of page 3 and ending at line 12, page 4, Prof. Hansen expressly states that his calculation of the secular acceleration proceeds on the assumption that $\Xi = 0$, or in other words that this term has no secular variation.

Now since

$$\Xi = -1 - \frac{h_0}{h} + 2\frac{h}{h_0} - \frac{3e_0}{1-e_0^2}\frac{h}{h_0}\{e\cos(\chi - n_0 y t - \varpi_0) - e_0\}\ddagger$$

* Abstract, *Monthly Notices*, vol. xxiii., p. 210.

† See the quotation from Laplace, page 164.

‡ Hansen's *Darlegung*, part i, art. 88, p. 332.

this is equivalent to assuming that $\frac{h_0}{h}$ has no secular variation, or that the Moon's mean areal velocity has no secular variation, since the quantity which Hansen denotes by h is inversely proportional to the areal velocity.* Accordingly, in part ii. Art. 305, p. 339, the value found for $\Delta \frac{h_0}{h}$ contains no non-periodic term, and the terms which would have naturally led to the introduction of such non-periodic term are expressly neglected, because they would produce an effect on the element Ξ, whereas it is assumed that, in the calculation of the secular acceleration, this element may be put $= 0$. This, at all events, would appear to be implied by the passage near the beginning of Art. 305: "*Und lässt dabei die Glieder, die kein t ausserhalb des Cosinuszeichens haben, weg, die später nur in dem Element Ξ Wirkung äussern können,und daher zufolge der Annahme $\Xi = 0$ in Bezug auf die Säcularänderung der mittleren Länge hier wirkungslos bleiben.*" The only reason for this assumption that $\Xi = 0$, which Prof. Hansen mentions in the passage at page 4 of his 2d part above referred to, is the agreement between his value of the secular acceleration and that given by the ancient eclipses.† But it is evident that the secular part of Ξ should be determined by theoretical considerations alone.

It is a purely mathematical process which leads to the values of the quantities contained in Ξ, and it is by the substitution of these values in this element that it must be determined whether Ξ is equal to zero, or whether it has any other value. It is evident that observation cannot decide whether a mathematical process is correct or not, for the very question is, whether the whole of the effect shown by observation is due to the cause which has been taken into account in the mathematical theory, or whether part of it may be due to some other cause.

Indeed it now appears, from a letter which Professor Hansen has done me the honour to communicate to me, under date of Jan. 29 of this year, that some misapprehension has existed in respect of the interpretation which has been put upon his meaning hitherto, for he says, "I have never contested the ideas of Mr. Adams in regard to the secular equation of the

* Hansen's *Darlegung*, part i., p. 106.

† In Art. 298, p. 322 of the 2nd part of the *Darlegung*, M. Hansen refers to his second memoir "On the Perturbations of the Small Planets" for a proof that Ξ can contribute nothing to the secular variations; but it should be remarked that this proof only takes into account terms which are of the *second* order with respect to the disturbing forces, whereas the terms which Mr. Adams has added to the Moon's secular acceleration are of the *third* order with respect to the disturbing forces, since these terms are of the the order of the square of the Sun's disturbing force multiplied by the change of the excentricity of the Earth's orbit, and this change is itself of the order of the disturbing forces of the planets on the Earth.

Moon, but I could not make use of the coefficient derived from his investigation, because it does not accord with observation. It is an inevitable condition which must be fulfilled, in calculating tables of the Moon or the planets, that they represent observations as far as possible. And if in any point the theory does not satisfy this condition, we must change the corresponding coefficient."* From this important explanation of Professor Hansen, it becomes all the more necessary to fix the dates and localities of ancient eclipses with the utmost attainable precision. For the attempts which have been made with more or less success in this direction we are under great obligation to Professor Hansen† himself, to the Astronomer Royal,‡ to Prof. Hansteen,§ and among the pioneers in this now increasingly important branch of inquiry, Mr. Bosanquet must be placed in the foremost rank.‖

Thus the value of the lunar acceleration due to the change in the excentricity of the Earth's orbit, which has been determined by Mr. Adams and M. Delaunay, may be regarded as established beyond all doubt, while at the same time it seems incontestable that a larger value is required in order to satisfy the ancient eclipses.

M. Delaunay has recently endeavoured to remove this difficulty by showing that there is a physical cause in operation adequate to explain the difference between the theoretical and the observed values of the acceleration. In a memoir read before the *Académie des Sciences*, on the 11th Dec. 1865,¶ he developes the following proposition:—The disturbing forces, to which are due the periodical oscillations of the surface of the seas, by their action on the liquid protuberances to which they give rise, tend to diminish the motion of the rotation of the Earth, and thus produce a sensible apparent acceleration in the mean motion of the Moon.—This communication is worthy of the earnest consideration of all physical astronomers; and although it is possible that immediate adherence may not be given to M. Delaunay's theory, it is sure to command the attention of those best qualified to discuss its merits.

That the tidal wave tends to cause a retardation of the Earth's rotation has been pointed out by Dr. J. R. Mayer, of Heilbronn, in his *Celestial Physics*, 1848;** and this action of

* See Appendix D.

† Hansen's *Darlegung*, 2nd part, pp. 386, 392, 398. See also Appendix C.

‡ *Phil. Trans.* 1853, *Mem. R. A. Soc.* 1858.

§ *Ergänzungs-Heft* to the *Astron. Nach.*, published in 1849.

‖ See Appendix E.

¶ *Comptes Rendus*, tome lxi. Séance du 11 Dec., 1865:—see also Abstract *Month. Not.* vol. xxvi. p. 85.

** *Beiträge zur Dynamik des Himmels, in Populärer Darstellung.* Heilbronn, 1848. Translated by Dr. H. Debus, *Phil. Mag.*, fourth series, 1863, vol. xxv., p. 403.

the Moon has been made familiar to the English public by Dr. Tyndall's *Lectures on Heat.*

It is not my intention to enter upon the merits of the Memoir of M. Delaunay in this address, but I deem it right to point out that while Mayer and others have seen that the tidal wave must act as a sort of brake tending to retard the rotation of the Earth, this illustrious French mathematician is the first who has attempted to prove that the effect is an appreciable quantity, *and such as could account for that part of the lunar acceleration which is not produced by the secular change in the excentricity of the Earth's orbit.* This is a grand result, and if confirmed, it must be ranked amongst the most important contributions to astronomical physics.

The secular change in the excentricity of the Earth's orbit, although of long period, is, after all, periodic, and hence that part of the acceleration of the Moon's mean motion which is dependent upon it is also periodic. This is not the case in respect of that part (the apparent acceleration) which arises from the increase of the time of the Earth's rotation, for that part will go on accumulating for ever; countless ages will, however, pass away before the length of the day will be so altered as to change in an appreciable degree the conditions of things upon the Earth. But the exact calculation of the effect of the tides in retarding the Earth's rotation is fraught with such great difficulties that it is most probable that its amount will be best obtained by comparing the whole observed amount of the lunar acceleration with that part of it which is theoretically computed from the reflex action of the planets, the primary effect of which is to change the excentricity of the Earth's orbit.*

We thus see that the re-examination of Laplace's theory of the Secular Equation by Mr. Adams has indirectly led to a more formal investigation of how far the effect of the Sun and Moon on the protuberance of the great tidal wave may produce a retardation of the Earth's rotation equivalent to the difference between Hansen's coefficient $12''{\cdot}557$, and Adams' and Delaunay's value $6''{\cdot}11$.

It should be observed that the question of what part of the acceleration is real and due to the Sun's action, and what part is merely apparent and due to a change in the time of the Earth's rotation, is by no means without practical importance.

For the part of the acceleration due to the first of the above-mentioned causes is peculiar to the Moon, whereas the part due to the other cause affects the places of all celestial bodies in proportion to their angular velocities. Thus, if the change in the time of the Earth's rotation produces an

* See Appendix F.

apparent acceleration of 6″ in a century in the motion of the Moon, the same cause must produce an acceleration of nearly 0″·5 in a century in the motion of the Earth (or in the apparent motion of the Sun), and nearly 2″ in a century in the heliocentric motion of *Mercury*.

I have now to speak of very important investigations of Professor Adams', in respect of the Moon's Parallax. Though I do not claim for these researches the same originality of ideas and method which characterised the memoir on the Secular Acceleration of the Moon's Motion, yet they are the representatives of a vast amount of labour, and are in the highest degree creditable to the sagacity and industry of the author.

It is needless for me to dilate on the importance of an accurate knowledge of the lunar parallax, for no observation of the Moon's place can be compared with the theoretical place as derived from the tables, without a previous reduction, wherein the question of the amount of parallax has first to be taken into consideration. Mr. Adams, in examining Burckhardt's Tables, found that those relating to the lunar parallax were defective.

The principal results of Mr. Adams' investigations respecting the Moon's parallax are contained in a paper "On new Tables of the Moon's Parallax, to be substituted for those of Burckhardt," which was published in the Supplement to the *Nautical Almanac* for 1856,* and in another paper, "On the Corrections to be applied to Burckhardt's and Plana's Parallax of the Moon, expressed in terms of the Mean arguments," which appeared in the *Monthly Notices of the R. A. S.*, vol. xiii. p. 262.

Mr. Adams' attention was called to the discrepancies between the values of the Moon's parallax, as calculated from the Tables of Burckhardt and of Damoiseau, by reading Mr. Henderson's paper on the Constant of Lunar Parallax in the tenth vol. of the *Memoirs of the R. A. S.* Mr. Henderson's determination of this constant is based on thirty-four observations of his own, made at the Cape of Good Hope, combined with corresponding observations at Greenwich and Cambridge. The values of the parallax deduced from the observations are compared with those calculated by means of the Tables, both of Burckhardt and of Damoiseau.

Now it is remarkable that the values of the constant of parallax thus found differ by 1″·3, according as one set of Tables or the other is employed in the comparison. Not knowing which of these values to prefer, Mr. Henderson adopts the mean of the two for his final result. A little consideration, however, will suffice to show the unsatisfactory

* Mr. Adams' Tables of the Moon's Parallax have been reprinted in the *Addit. à la Conn. des Temps* for 1856, and also in the *Berliner Jahrbach* for the same year.

nature of this mode of procedure. It is to be remarked that the only part of the investigation which is dependent on the Tables consists in the reduction from the actual values of the parallax at the times of observation, to the constant or the mean value of the parallax. Now it is plain that so large a difference as 1″·3 in the mean amount of these reductions corresponding to the thirty-four observations concerned, can only arise from intolerable errors in the periodic terms of the parallax given by one of the two sets of Tables. The two results must, therefore, be regarded as contradictory of each other, and must not be combined together. One of the results may be good, but if so, it is sure to be vitiated by combination with the other which must be decidedly erroneous.

Damoiseau's Tables give the parallax at once in the form in which it is furnished by theory, but the expression of the parallax which is used in Burckhardt's Tables is adapted to his peculiar form of the arguments, and requires transformation in order to be compared with the former. When this transformation had been effected, Mr. Adams found that several of the minor equations of the parallax deduced from Burckhardt differed essentially from their theoretical values given by Damoiseau. With regard to the parallax, Burckhardt professes to have followed the theory of Laplace, but as this agrees very closely with that of Damoiseau, it is clear that errors must have crept into Burckhardt's transformation of Laplace's formula. These errors appear to have arisen in the following manner.

In the formation of Burckhardt's arguments of evection and variation, the mean longitude of the Sun is employed. Now, four of the errors in the coefficients of the minor equations may be accounted for, by supposing him to have erroneously employed the true instead of the mean longitude of the Sun in forming the above-mentioned arguments. In another of the minor equations, the coefficient is taken with a wrong sign, and in another a wrong argument is employed.

On further inquiry, Mr. Adams discovered that the difference between Burckhardt's equations of parallax and those of Burg and Damoiseau had been long since remarked by Clausen in a valuable comparative analysis of the three sets of lunar tables given in the 17th volume of the *Astronomische Nachrichten*, but this remark seems to have excited no attention whatever.

After examining Burckhardt's Tables of Parallax, Mr. Adams was naturally led to scrutinize more closely the results of the theories of Damoiseau, Plana, and M. de Pontécoulant, in relation to the same subject. Although the differences between the results of the several theories were, in general, small when compared with the errors of Burckhardt, still they were greater than we had a right to expect would have been the case, considering the close agreement which existed

between the several theories with reference to the equations of longitude.

On examining the processes which had been employed by the several authors, he found that the numerical values of the coefficients of the equations of parallax assigned by them by no means fairly represented the most accurate values of those coefficients which the several theories were capable of giving.

In the theories of Damoiseau and Plana, the expression for the reciprocal of the projection of the Moon's radius vector upon the plane of the ecliptic in terms of the Moon's true longitude, is required in order to find the relation between that longitude and the time, and consequently the utmost care has been taken to obtain that expression with accuracy.

In the subsequent operations and transformations necessary in order to deduce the expression for the parallax, first, in terms of the Moon's true longitude, and finally, in terms of the time, the same care has by no means been employed. In Damoiseau's theory, the coefficients of the expression for the reciprocal of the projected radius vector are only given numerically, and the quantities neglected in the subsequent transformations are not very important, although they are still sensible. In Plana's theory, the coefficients of the same expression are given in their analytical form, and the approximations are carried to the seventh, or even to the eighth order of small quantities, in cases where this degree of accuracy is requisite. In the transformations for finding the parallax, however, Plana neglects all quantities which are of higher orders than the fifth, and consequently many of his coefficients are very sensibly in error.

In M. de Pontécoulant's theory, the time is taken as the independent variable, and the analytical expression for the parallax in terms of the time is obtained immediately, and is developed to as great an extent as the corresponding expression for the longitude. In the conversion of his formulæ into numbers, he, however, neglects all the terms beyond the fifth order, and therefore many of his coefficients, like Plana's, are sensibly erroneous.

Mr. Adams has endeavoured to supply the defects and omissions in the several theories which have been above pointed out. He has transformed anew Damoiseau's and Plana's expressions for the reciprocal of the projected radius vector, so as to obtain the expression for the parallax in terms of the time, with all the accuracy of which the respective theories admit, and he has also converted into numbers the terms of M. de Pontécoulant's expression for the parallax which have been neglected by M. de Pontécoulant himself.

In the course of these operations, Mr. Adams has succeeded in detecting and tracing back to their sources a considerable number of errors, especially in Plana's work, and has thus

been able to remove many of the discrepancies between the results of Baron Plana and M. de Pontécoulant.

When the values of the parallax given by the several theories had been corrected in the manner above described, the agreement between them was found to be most satisfactory. The difference between the separate values of each coefficient of parallax and the mean of all, rarely amounted to a hundredth part of a second.

As an example of the differences between the values of the coefficients of parallax originally given by the several authors, we may take the coefficient of the principal term in the variation inequality, the argument of which, expressed in Damoiseau's notation, is $2t$.

The several values given for this coefficient are the following :—

	″
Damoiseau	28·54
Plana	27·59
Pontécoulant	27·475

whereas all the theories, when corrected, agree in giving the value

$$28''\cdot23$$

In one of Plana's coefficients the error amounts to not less than $1''\cdot9$, but this simply arises from the principal term in the analytical expression of this coefficient having been inadvertently taken with a wrong sign, in the conversion of the formula into numbers.

Mr. Adams has taken particular pains with the theoretical determination of the Constant of Parallax, and with regard to this the agreement of the several theories is perfect. From Dr. Peters' value of the Constant of Nutation, it may be deduced that the ratio of the Earth's mass to that of the Moon is as 81·5 to 1 very nearly. Employing this ratio, together with the dimensions of the Earth according to Bessel, and the length of the seconds' pendulum in latitude $35\frac{1}{4}°$, deduced from Mr. Baily's Report on Foster's Pendulum Experiments, he finds the value of the Constant of Parallax to be $3422''\cdot325$.

This result agrees admirably with that found by Mr. Henderson, in the paper cited above, when the observations are reduced by means of Damoiseau's Tables.

In the seventeenth volume of the *Astronomische Nachrichten*, M. Hansen gave the value of the parallax which he had at that time obtained by means of his new method of treating the lunar theory. Mr. Adams transformed this expression with great care, so as to compare it with the results of the former theories. The agreement is in general very close; the difference between the values of the co-efficients seldom exceeding a hundredth part of a second.

There are only two instances in which the difference is much greater than this quantity. One of these cases is that of the Constant of Parallax, the value of which, given by M. Hansen's theory, is 0″·06 less than the corresponding value found from the same fundamental data by the other theories. The second case is that of the term whose argument expressed in Damoiseau's notation is $t+z$, the co-efficient being 0″·146 according to Damoiseau and Plana, 0″·140 according to Pontécoulant, and 0″·181 according to Hansen.

In both these cases Mr. Adams finds that Hansen's definitive values of the co-efficients as given in his Lunar Tables differ sensibly from those given in the paper above referred to, and agree closely with those which result from the former theories.

In the formulæ given in Mr. Adams' published papers on Parallax, referred to at the beginning of this notice, quantities less than 0″·05 have been neglected, except in cases where they can be included in the same table with larger terms. In Mr. Adams' computations, however, quantities far smaller than this have been taken into account, and an additional place of decimals has been employed, beyond that retained in the results.

The total error in the periodic terms of Burckhardt's Tables of Parallax may amount to nearly 6″, and independently of this his constant of parallax requires an increase of nearly 2″.

It is unfortunate that so erroneous a value of the Parallax, as that of Burckhardt, should have been so long employed in the *Nautical Almanac*, and consequently should have been used in the reduction of such a vast number of lunar observations.

In the Appendix to the *Nautical Almanac* for 1856, which contains Mr. Adams' Tables of the Parallax, are also given tables of corrections to be applied to the values of the Parallax given in the *Nautical Almanac*, for every day in each of the years from 1840 to 1855 inclusive. From 1856 to 1861 inclusive, the values of the Parallax given in the *Nautical Almanac* were computed by means of his tables. Subsequently to that time Hansen's Tables have been employed.

In Mr. Airy's "Reductions of the Greenwich Lunar Observations," from 1750 to 1830, Plana's formula for the Parallax has been employed. We have already seen that this formula requires corrections which are by no means insensible.

I could quote largely from my friend and a former occupier of this Chair, the Rev. R. Main, in elucidation of the vast amount of trouble which has been occasioned by the artificial arguments which had been devised by Burckhardt in the construction of his tables, but Mr. Main's lucid explanation of the claims of that fine old veteran Hansen to your gratitude, are so vivid in your recollection that it is needless for me to do so.

The address which he delivered in 1860 contains so much of interest that it is worthy of re-perusal now that we are dealing with the utmost refinements of the lunar theory. My own experience has led to the opinion, often expressed, that a large portion of time is taken up in the correction of errors, and this remark most certainly applies to practical astronomy; most grateful ought we, therefore, to be to those men who, with zeal and untiring industry, occupy themselves in labours such as those to which I have called attention, and who by their sagacity have saved us from the uncertainty and useless toil invariably entailed by the use of erroneous tables.

Besides the works before mentioned, there are several investigations by Mr. Adams, the results of which have been published without the processes which have led to them; and it will not be considered out of place to direct attention to these works, although they do not constitute any part of the grounds on which the award of the Medal was made. One of these is an investigation of the relative positions in space of the two heads of Biela's Comet, as deduced by means of a method of his own from the apparent relative positions observed at Cambridge. This investigation showed that the relative motion during a month might be fully accounted for without supposing any sensible mutual action between the two heads. The periodic time of the smaller head was found to be 8·48 days longer than the periodic time of the larger. The results of this interesting investigation were published in the *Monthly Notices*, vol. vii. p. 83 (April 1846).

Among the works of Mr. Adams I find in the *Nautical Almanac* for 1851, a correction to Bouvard's Table XLII., of *Saturn*. The re-examination of the theory of this planet was suggested by the discordances between the computed and observed places exhibited in the *Greenwich Planetary Reductions*. No higher testimony to the practical value of this investigation is needed than to state that the results are now employed by Mr. Hind in the computation of the places of *Saturn* for the *Nautical Almanac*.

Another investigation involving considerable labour is that relating to the mass of *Uranus*, which will be found in the *Monthly Notices*, vol. ix. p. 159. Two values of this mass had been previously given, differing widely from each other. Bouvard, from the action of *Uranus* on *Saturn*, found the mass to be $\frac{1}{17918}$ of the Sun's mass; while more recently, from observations of the satellites, Lamont obtained the value $\frac{1}{24605}$. From a careful reduction of Mr. Lassell's observations of the fourth satellite (which are more to be depended on for this purpose than those of the second) Mr. Adams obtained the value $\frac{1}{20897}$, which is almost exactly a mean between the results of Bouvard and Lamont. He also reduced all Sir Wm. Herschel's measures of distance of the satellites, given in his paper in the *Phil. Trans.* 1815, and obtained from the ob-

servations of the fourth satellite the quantity $\frac{1}{2\,1\,0\,5}$, which agrees closely with that found from Mr. Lassell's observations. Mr. Adams arrived at the conclusion that $\frac{1}{2\,1\,0\,0\,0}$ is probably much nearer the true value of the mass than either of the values that had been previously given, and that it may be employed, provisionally at least, in the theory of *Neptune*, without risk of any considerable error.

In Admiral Smyth's *Ædes Hartwellianæ*, p. 341, and also in his *Speculum Hartwellianum*, p. 364, are published Mr. Adams' results respecting the orbit of the double star γ *Virginis*. The orbit given by Sir John Herschel, in the results of his *Cape Observations*, was taken as the basis of the calculations; and equations of condition for the correction of the elements were formed, by comparing certain selected angles of position deduced from observation with the values calculated by means of Sir John Herschel's elements. These equations of condition were formed by a method of Mr. Adams' own, which appears to be more simple than any other before used.

Laplace has said, that every difficulty which has arisen in explaining the inequalities of planetary movements has ultimately served to establish on a firmer basis that most brilliant discovery of Newton,—the law of Universal Gravitation. Among those ardent and illustrious mathematicians, who have contributed towards the clearing away of these difficulties, there is none who stands higher than the recipient of the Medal this day, whose name is and ever will be associated with that grand investigation of the perturbations of *Uranus*, by an unknown planet, with which he began his career. It does not come within the purpose of my present address to enter upon the connexion of Professor Adams with the discovery of the planet *Neptune;* it would suffice to say that those most competent to judge of the merits of this celebrated investigation have always been greatly impressed by the power and comprehensiveness with which, from beginning to end, he grasps all the bearings of this difficult subject, as well as by the clearness, directness of aim and precision, which characterize his whole treatise:—but I cannot refrain from recalling those memorable words uttered by Sir John Herschel, who, in speaking of Leverrier and Adams in connexion with that discovery, declared theirs to be "names which Genius and Destiny had joined," to "be pronounced together so long as language shall celebrate the triumphs of science in her sublimest walks."

And now, Professor Adams, it only remains for me to congratulate you most heartily on the full confirmation which your theory of the Secular Acceleration of the Moon's mean motion has received at the hands of so many distinguished analysts, and in presenting you with this Medal to express a hope that your health may long permit you to exercise that great intellect with which you have been endowed, and which you have so highly and so successfully cultivated.

Appendix A.

(See *Monthly Notices*, vol. xx. commencing at p. 230.)

In Mr. Adams' "Reply to Various Objections" he points out that the error of M. Plana's original value of the secular acceleration, given in his "*Théorie du Mouvement de la Lune*," arises from his assumption that one of the so-called constant quantities introduced by integration is absolutely constant, whereas it is really subject to a secular variation. He also shows that the value of the acceleration given by M. de Pontécoulant in the *Comptes Rendus* of April 9, 1860, differing widely as it does from that of M. Plana, is vitiated by an assumption of exactly the same kind with respect to another constant introduced by integration.

In M. Plana's theory a constant is introduced which is denoted by h^2.

In M. de Pontécoulant's theory two constants are introduced, which are denoted by h and $\frac{1}{a}$ respectively.

These constants are not independent, but are connected with each other and with the Moon's mean motion n by the following relations:—

$$h = n^{-\frac{1}{3}} \left\{ 1 - \frac{1}{3}m^2 + \frac{719}{576}m^4 + e'^2\left[-\frac{1}{2}m^2 + \frac{2635}{384}m^4 \right] \right\}$$

$$h = n^{-\frac{1}{3}} \left\{ 1 - \frac{1}{3}m^2 + \frac{11}{144}m^4 + e'^2\left[-\frac{1}{2}m^2 - \frac{185}{96}m^4 \right] \right\}$$

$$\frac{1}{a} = n^{\frac{2}{3}} \left\{ 1 + \frac{2}{3}m^2 - \frac{1253}{288}m^4 + e'^2\left[m^2 - \frac{5593m^4}{192} \right] \right\}$$

in which formulæ the sum of the masses of the Earth and Moon is supposed to be unity, the excentricity and inclination of the Moon's orbit are neglected, and powers of m above the fourth are omitted.

By differentiating these relations and observing that $\frac{dm}{m\,dt} = -\frac{dn}{n\,dt}$ since $m\,n$, which is the Sun's mean motion, is constant, we may obtain,

$$\frac{dn}{n\,dt} = -3\frac{dh}{h\,dt} + \frac{d(e'^2)}{dt}\left\{ -\frac{3}{2}m^2 + \frac{2187}{128}m^4 \right\}$$

$$\frac{dn}{n\,dt} = -3\frac{dh}{h\,dt} + \frac{d(e'^2)}{dt}\left\{ -\frac{3}{2}m^2 - \frac{297}{32}m^4 \right\}$$

$$\frac{dn}{n\,dt} = -\frac{3}{2}\frac{da}{a\,dt} + \frac{d(e'^2)}{dt}\left\{ -\frac{3}{2}m^2 + \frac{5337}{128}m^4 \right\}$$

If $\frac{d\,\mathrm{h}}{\mathrm{h}\,d\,t}$ be neglected, or h be supposed to be constant, we obtain the value of $\frac{d\,n}{n\,d\,t}$ found in M. Plana's theory.

If $\frac{d\,a}{a\,d\,t}$ be neglected, or a be supposed to be constant, we obtain the value of $\frac{d\,n}{n\,d\,t}$ found by M. de Pontécoulant, in his paper in the *Comptes Rendus* above referred to.

If $\frac{d\,h}{h\,d\,t}$ be neglected, we obtain the value of $\frac{d\,n}{n\,d\,t}$, which would have been found by M. de Pontécoulant, if he had supposed the quantity h to be absolutely constant instead of the quantity a.

The several values of $\frac{d\,n}{n\,d\,t}$ thus obtained are evidently contradictory, and the reason is, that the quanties h, h, and a, are really variable, and therefore $\frac{d\,\mathrm{h}}{\mathrm{h}\,d\,t}$, $\frac{d\,h}{h\,d\,t}$, and $\frac{d\,a}{a\,d\,t}$, cannot be neglected. If the values of these differential coefficients be found in the way which Mr. Adams points out, and then substituted in the above equations, we find from all alike the value

$$\frac{d\,n}{n\,d\,t} = \frac{d\,(e'^2)}{d\,t}\left\{-\frac{3}{2}m^2 + \frac{3771}{64}m^4\right\}$$

which is the result obtained by Mr. Adams.

Damoiseau's value of the acceleration is not expressed in an analytical form, but his method of obtaining it is exactly equivalent to that of M. Plana.

M. Hansen, as is well known, does not express the lunar inequalities in terms of m, but, from the remarks which have been already made in this Address respecting his method of finding the acceleration, it appears probable that his process is equivalent to neglecting $\frac{d\,h}{h\,d\,t}$ or assuming that the quantity h is absolutely constant.

Appendix B.

For the following tabular view of the results of Mr. Hartwig I am indebted to Mr. Marth, who remarks that Nos. 15 and 19 are favourable to Mr. Adams's acceleration, but that all the others are opposed to it. The prevailing sign of the differences between the recorded and computed times appears to favour a diminution of the coefficient for the secular acceleration employed in the tables of about 2″. The necessity however of rendering eclipse No. 8 physically possible (assuming that there is no error in the date) seems strongly opposed to such diminution.

No.	Date.	Place.	Greatest Phase: Recorded.	Greatest Phase: Cal.	Recorded Time.	Cal. Time	Diff.
						h m	
1	— 720 March 19	Babylon	total	1·62	Beg. a good hour after moonrise (which occurred towards 6^h).	7 8	
2	— 719 March 8	,,	$\frac{1}{4}$	0·21	Middle uncertain, about midnight.	11 13	
3	— 719 Sept. 1	,,	more than $\frac{1}{8}$	0·57	Beg. after moonrise (towards $6\frac{1}{2}^h$).	6 14	
4	— 620 April 21	,,	$\frac{1}{4}$	0·07	Beg. $16^h\ 37^m$	15 56	-41^m
5	— 522 July 16	,,	$\frac{1}{2}$	0·45	Mid. uncertain, an hour before midnight.	11 2	
6	— 501 Nov. 19	,,	$\frac{1}{4}$	0·11	Mid. uncertain, about $11^h\ 20^m$.	11 24	
7	— 490 April 25	,,	$\frac{1}{6}$	0·02	Mid. uncertain, about $11^h\ 28^m$.	10 14	
	— 424 Oct. 9	Athens	total	1·34			
	— 412 Aug. 27	Syracuse	,,	1·16			
	— 405 April 15	Athens	,,	1·34			
8	— 382 Dec. 22	Babylon	...	...	Beg. while half an hour of the night was yet left ($18^h\ 31^m$).	18 53	+22
					h m		
9	— 381 June 18	,,	...	...	Beg. 7 47	25	−22
10	— 381 Dec. 12	,,	total	1·42	Beg. 9 36	8 55	−41
11	— 200 Sept. 22	Alexandria	...	...	End. 8 26	7 57	−29
12	— 199 March 19	,,	total	1·44	Beg. 11 28	10 54	−34
13	— 199 Sept. 11	,,	total	1·65	Beg. 12 36 Mid. 14 11	12 15 14 6	−21 − 5
14	— 173 April 30	,,	$\frac{7}{12}$	0·67	Beg. 12 49 Mid. 15 32	12 8 14 50	−41 −42
15	— 140 Jan. 27	Rhodes	$\frac{1}{4}$	0·29	Beg. 9 56	8 41	−75
16	125 April 5	Alexandria	$\frac{1}{6}$	0·11	Mid. uncertain, about $8^h\ 26^m$.	8 39	
17	133 May 6	,,	total	1·05	Mid. 11 8	10 41	−27
18	134 Oct. 20	,,	$\frac{5}{6}$	0·88	Mid. 10 47	10 37	−10
19	136 March 5	,,	$\frac{1}{2}$	0·43	Mid. 16 13	15 29	−44

Appendix C.

I am indebted to Mr. Marth for the following notes referring to Hansen's Calculations of old Solar Eclipses.

The value of the acceleration of mean longitude employed in Hansen's Lunar Tables is $12''\cdot180$. His further researches (vide *Darlegung der theoretischen Berechnung*, &c., 2nd part, page 374) have led him to increase this value by $0''\cdot377$, and he has found, by extending his researches, corrections of the tabular retardation of the perigee and of the acceleration of the node.

	Hansen's Lunar Tables.	Final Values.	Diff.
Secular Variation of Mean Longitude	$+12''\cdot180\,t^2$	$+12''\cdot557\,t^2$	$+0\cdot377$
,, Perigee	$-37\cdot255\,t^2$	$-38\cdot577\,t^2$	$-1\cdot322$
,, Node	$+7\cdot068\,t^2$	$+6\cdot623\,t^2$	$-0\cdot445$

The tracks of the old total Solar Eclipses (arranged in chronological order) are altered by these corrections in the following manner:—

1. Eclipse of Thales, -584 May 28.
   The zone of totality is thrown $1^\circ\,7'$ towards the north, and covers the greater part of the whole region within which it may be fairly assumed that the battle between the Lydians and Medes must have taken place. It covers, also, the Hellespont, and satisfies the remark of Theon (in his commentary on Ptolemy's *Almagest*), who speaks of an eclipse which was total in the Hellespont and its neighbourhood, while at Alexandria the greatest phase was at most $\frac{4}{5}$ths of the Sun's diameter. The northern limit of the zone of totality approaches also to Boghaskoei, lat. $40^\circ\,2'$ N., long. $34^\circ\,21'$ E., in the neighbourhood of which monuments cut in the rocks have been found, appearing to refer to the war between the Lydians and Medes, to the solar eclipse, and to the succeeding events.

2. Eclipse of Larissa, -556 May 19.
   The central track given by the tables passes $1'$ north of Larissa. Hansen's final values throw the track $8'$ more north, and the eclipse remains total.

3. Eclipse of Ennius, mentioned by Cicero, -399 June 21.
   The tables make the eclipse total for Rome, 5^m before sunset. Hansen's corrections give the greatest phase $0\cdot987$ (Sun's diameter $= 1$) 12^m before sunset, which seems quite compatible with the record: "Anno ccc quinquagesimo fere post Romam conditam Nonis Juniis soli luna obstitit et nox." (*Cic. De Republica*, I. 16.)

4. Eclipse of Agathocles, -309 August 14.
   The south-east point of Sicily, Cape Passaro, is about $45'$ distant from the northern limit of the zone of totality as given by the tables. Hansen's corrections increase the distance to about $51'$. In the uncertainty of Agathocles' real position at the time of the eclipse, we are left to conjecture how near he may have been to the zone of totality.

5. Eclipse of Stiklastad, 1030 August 31.
   The shortest distance of Stiklastad from the central track, as given by the tables, is $1^\circ\,20'$, and from the northern limit of the zone of totality $1^\circ\,9'$ (or, in case the ellipticity of the Earth is assumed to be $\frac{1}{294}$ instead of $\frac{1}{300}$, about $2'$ less). Hansen's corrections reduce these distances to

1° 11′ and 59′, and increase the greatest phase at Stiklastad from 0·985 to 0·988.

The corrected tabular values represent also—

6. The Eclipse of Rome, 1567, April 9, giving 1″·1 as the greatest breadth of the uncovered part of the Sun, as seen from the Collegium Romanum. Keppler's record—"Clavius in commentario in Sacroboscum recenset, anno 1567, Apr. 9, Romæ Solem non totum defecisse, sed relictum fuisse exilem quendam circulum lucentem circumcirca"—might indeed suggest an annular eclipse; it was however total in the neighbourhood of Rome, though only for a very short time.

If the retrograde motion of the node is less rapid by 12″ in a century than that of the Tables, an assumption which would be compatible with Bradley's observations, it would satisfy the circumstances of some of the old eclipses perhaps still better. The track of the eclipse of Thales would go still farther north; the eclipse of Larissa would remain total, though Larissa would now be near the northern limit; the eclipse of Ennius would have a greatest phase of 0·979, 11 minutes before sunset; the track of the eclipse of Agathocles would be thrown farther north, so that the northern limit of the zone of totality would pass at a distance of 28′ from Cape Passaro. Stiklastad would approach within 40′ to the northern limit of totality.

On the other hand, a diminution of Hansen's tabular acceleration of mean longitude by 6″ would not satisfy any of the old records (supposing that the years are correct). Accepting his final values of the secular variations of the perigee and node, the track of the eclipse of −584 (Thales) would be thrown more than 9° southwards, and would not reach the regions of the battle before sunset; the greatest phase of the eclipse of −556 would not exceed $\frac{2}{3}$ of the Sun's diameter at Larissa, and that at sunset; the eclipse of Ennius would not be at all visible at Rome, as it would happen after sunset; the southern boundary of totality of the eclipse of −309 (Agathocles) would be thrown to about 40′ north of Syracuse; the track of the eclipse of Stiklastad would be thrown 1° 18′ more south.

Appendix D.

At my request Professor Hansen has been so good as to communicate to me the following explanation of his reasons for using the larger coefficient of the lunar acceleration in his Lunar Tables:—

"I have never disputed the correctness of the Theory of the Secular Equation of the Moon's Longitude, such as Mr. Adams was the first to propound; but I am not satisfied with the development of the divisors into series, of which he has made use, as several other geometers have also done. As the coefficient which results from Mr. Adams' theory does not accord with observations, it could not be employed for the Lunar Tables: for in the construction of tables, either planetary or lunar, the first condition to be fulfilled, is to construct them in such manner that they represent observations as closely as possible, for without this they would be of no practical value, and therefore useless.

"Mr. Adams' theory came too late to permit of my making use of it in my works; and it was well that it so happened, for I had already found by my own theory a coefficient which represents ancient observations as closely as could be desired; which is not the case with Mr. Adams' coefficient.

"As my two Memoirs, entitled *Darlegung*, &c. were destined to the development of the calculations by which I had arrived at the coefficients used in my Lunar Tables, I could not therein employ the theory of Mr. Adams; and in the introduction to the Second Memoir, p. 4, I have explained myself on this point, adding that ulterior researches on this subject were to be reserved for a special memoir. Subsequently I endeavoured to account for the fact that Mr. Adams' coefficient did not accord with observation, and I found that an extremely small retardation in the rotation of the Earth on its axis sufficed to explain it; this was published in the *Berichte der K. Sächs. Gesellschaft zu Leipzig* for 1863, in the note to which you have drawn attention. Already M. Mayer, of Heilbronn, in his *Beiträge zur Dynamik des Himmels, etc.*, a work with which at the time I was unacquainted, had directed the attention of geometers to the Tidal Wave as capable of causing such a retardation of the Earth's rotation; and recently M. Delaunay has submitted this point to analysis.

"In the actual position of the question it may be concluded from all these researches, that the acceleration of the Moon's motion depends on two causes, that is to say, the decrease of the excentricity of the terrestrial orbit and a retardation of the rotation of the Earth on its axis, and that it is the combined effect of these two causes which produces the amount of acceleration which observations show.

"It is, however, very remarkable, that under all these circumstances, the hypothesis that $\Xi = 0$, in regard to the acceleration, leads to a value for the coefficient in question which represents the old eclipses as well as can be desired, and which for this reason ought to be considered as the true value of the coefficient for the secular equation of the mean longitude of the Moon.

"*Gotha*, 1866, *Feb.* 10."

"P. A. Hansen.

Appendix E.

Mr. Bosanquet deserves great credit for the zeal he has evinced in promoting the investigation of certain ancient eclipses. It appears that he was the first to call the attention of Astronomers, and more particularly that of Mr. Airy, to historical facts in connexion with the expedition of Agathocles, which it was necessary to take into account in interpreting that eclipse. Moreover, Mr. Bosanquet urged upon Mr. Airy's attention facts which induced him to search out and identify the important eclipse of Larissa; and he has brought forward evidence tending to show that the appearance occurring to Hezekiah* is connected with the solar eclipse which took place −688, Jan 11, when, he argues, that central conjunction took place about 20 minutes before noon, and that the eclipse was to the extent of about two-thirds of the Sun's diameter.

* "And Isaiah the prophet cried unto the Lord, and he brought the shadow ten degrees backward, by which it had gone down in the dial of Ahaz."—2 Kings, xx. 11.

Prof. Hansteen was the first to bring under notice the eclipse of Stiklastad, "which," says the Astronomer Royal, "in combination with that of Larissa, appears likely to throw light upon the correctness of the lunar tables."

The interest of Mr. Airy in these ancient eclipses is naturally concentrated upon the data they afford for determining elements connected with the lunar motions. A perusal of his investigations published in the *Philosophical Transactions* in 1853, and in the *Memoirs of the Royal Astronomical Society* in 1858, will repay all who take an interest in the history of the development of the lunar theory. Although the main object of the papers is the fixing of the time and place of the phenomena under discussion, with a view to the correction of lunar elements, yet certain matters of historical and geographical interest are not neglected. Among these may be cited the fixing of certain limits within which the battle occurred between the Lydians and Medes in Asia Minor —584, May 28; also of the landing-place of Agathocles on the African shore; lastly, the correction of the longitude of Nimrûd, as given in Capt. F. Jones' map.

Appendix F.

Suppose that Hansen's value is practically required, and that the difference between the theoretical and the practical value is caused entirely by the retardation of the Earth's rotation, Hansen, in the remarks already quoted, tacitly assumes, that, because the time of rotation becomes variable, therefore the length of the mean day, which is the unit of time of the tables, becomes also variable. Now, this is not at all such a matter of course. If astronomers agree to retain as the unit of time a mean day of *uniform* length, the whole effect of the retardation of rotation is thrown upon the tables for converting sidereal time into mean time, and the tables of the Moon and planets remain perfectly unaffected by the variability of the rotation, and the Lunar Tables must therefore contain the strictly theoretical value of the acceleration :—this course of taking account of the retardation seems to be more simple than the other.

Before retiring from the high and honourable position to which, for two successive years, you have been pleased to call me, and resigning into the able hands of my successor the responsible duties of the Presidency, I desire emphatically to assure you how fully I have always appreciated the generous support on all occasions accorded me by my Colleagues in the

Council, and by yourselves in this Room. I have never occupied this chair without feeling that I was in the presence of considerate friends cordially sympathising and co-operating with me in my continual, however imperfect, endeavours to make our meetings both instructive and agreeable, and ever ready to condone, with their indulgent kindness, my inevitable shortcomings. To each and to all I tender my grateful thanks.

During my tenure of office one or two innovations have been introduced, to which I now refer simply for the purpose of stating that they were purely personal, and in no way binding on any future occupant of the President's chair. One of these is, the report of the *vivâ voce* discussions at our meetings; the other is, the annual *Réunion* of our own Members and the members of other scientific bodies, as well as of other gentlemen of eminent position in political or social life. It was a matter of great gratification to me to meet so many distinguished friends and cultivators of science; and I believe that such assemblages are productive of good; but at the same time I wish it to be understood that I had no desire to establish a precedent.

And now, let me add that my two years of office have been to me a source of the highest and purest pleasure.

The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

President:

Rev. Charles Pritchard, M.A. F.R.S.

Vice-Presidents:

Rev. Professor Challis, M.A. F.R.S.
Warren De La Rue, Esq. F.R.S.
John Russell Hind, Esq., F.R.S., Superintendent of the *Nautical Almanac*.
Rev. Robert Main, M.A. F.R.S. Radcliffe Observer.

Treasurer:

Samuel Charles Whitbread, Esq., F.R.S.

Secretaries:

Richard Hodgson, Esq.
Edward J. Stone, Esq. M.A.

Foreign Secretary:

Admiral R. H. Manners.

Council:

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John Lee, Esq. LL.D. F.R.S.
Captain William Noble.
J. Norman Lockyer, Esq.
Major-Gen. Shortrede.

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